European University Institute

DEPARTMENT OF ECONOMICS

Essays on Bank Behaviour and Financial Regulation

Matic Petriček

Thesis submitted for assessment with a view to obtaining the degree of Doctor of Economics of the European University Institute

Florence, 18 January 2019

European University Institute
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I confirm that chapter 1 was jointly co-authored with Alessandro Ferrari, Carmen García Galindo and Andreas Winkler.

I confirm that chapter 2 was jointly co-authored with Carolina López-Quiles.

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10. 1. 2019

M.Petriček

Acknowledgements

This thesis brings an end to a four and a half year long journey. The end would not have been attainable without the support and guidance from my supervisors Juan Dolado and Árpád Ábrahám, so it is only fair that I start by thanking them. My gratitude extends also to the rest of the faculty and the administrative staff at the Economics Department of the European University Institute. Together they have established an environment where classes and office hours are followed by a coffee get-together and finished with an evening enjoying a football match at the Coppa.

This journey would not have been as enjoyable without friends that I have made among the students at the department, some with whom I have had an opportunity to coauthor with. Carolina López-Quiles and Andreas Winkler deserve a special gratitude for their contribution and their time spent solving seemingly unsolvable problems.

Finally, there are the two coauthors that I just did not manage to ditch at the department but instead followed me home every evening, Alessandro Ferrari and Carmen García Galindo. Nothing brings friends closer together like figuring out economics during the day and then failing to fry onion rings to have with burgers for dinner.

I devote this thesis to my family and Tina who have provided unconditional support. I have no words to express my gratitude except those which I have been longing to say. I am coming home.

Abstract

This thesis studies bank behaviour in response to financial regulation and monetary policy.

In the first chapter a novel approach to address issues of endogeneity in estimating a causal effect of leverage on risk taking by banks is used. By using data on local bank office deposits and local unemployment an instrument is constructed to use in a regression of leverage on a measure of risk taking constructed from new issuance of loans. The results are consistent with a theoretical prediction that due to limited liability banks increase their risk taking after an exogenous increase in leverage.

The second chapter estimates the effect of deposit insurance on the risk-taking behaviour of banks. As shown in the theoretical literature, deposit insurance may induce moral hazard and incentivise banks to take on more risk. This chapter provides an experimental setup in which an increase in the coverage limit of deposit insurance in the U.S. is exploited in order to identify the difference in risk taking by banks that were affected and banks that were not. This difference comes from the fact that state chartered savings banks in Massachusetts had unlimited deposit insurance coverage at the time when it was increased for all other banks in the US. Given that all banks in the sample are subject to the same regulatory and supervisory requirements, and that they are similar in other characteristics, the effect of such increase in deposit insurance can be isolated. The findings suggest that, contrary to the literature, an increase in deposit insurance did not increase bank risk-taking, nor did it affect market discipline, evident through a lack of effect on deposit rates.

Motivated by substantial differences in employment dynamics across different geographical areas and substantial differences across banks which operate in these geographical areas, the third chapter estimates the effect of characteristics of banks operating in a particular location on the impact of monetary policy on the local economic outcomes. The results suggest that the effect of monetary policy on local employment and local total payroll intensifies as the capital structure of local banks improves and the credit risk associated with local banks decreases. These findings go in line with a prediction that healthy banks find it easier to attain alternative sources of funding following a monetary tightening. The results also show that size and liquidity position of local banks does not affect the impact of monetary policy.

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Chapter 1

Bank Funding and Risk Taking

joint with C. Garcia Galindo, A. Ferrrari and A. Winkler

1.1 Introduction

There are two established, opposing theoretical results about the effect of leverage on risk taking by banks. First, due to limited liability, expected returns on equity investment increase with an increased riskiness of the portfolio. As a bank's equity holders are protected from the left tail of the returns to assets distribution by limited liability, they have an incentive to increase the variance of the distribution by taking on more risk. On the other hand, callable demand deposits constitute a substantial part of bank debt. On average, almost 20 percent of these are above the amount insured by the Federal deposit insurance corporation. This provides depositors with a strong incentive to monitor as well as with tools to punish excessive risk taking.

There is inconclusive empirical evidence on which of the two effects prevails. Some authors such as Koudstaal and van Wijnbergen (2012) find that higher leverage leads to less risk taking, while other such as Acosta Smith et al. (2017) find that an increase in capital requirements leads to a decrease in risk taking. Finally, there are some authors such as Jacques and Nigro (1997) that find no effect at all. As we argue below, the results in all these papers are, however, influenced by endogeneity issues. Our aim in this paper is then to estimate the causal effect of leverage on risk taking behaviour of banks. In doing so we add to the literature by proposing a novel approach to addressing endogeneity in this setting.

We identify two sources of endogeneity and propose a method to address them. First, an increase in leverage may incentivise banks to pursue riskier investment, but at the same time the demand and supply of deposits, which drive a bank's leverage given equity, can also be affected by a bank's risk taking, giving rise to reverse causality. Such an effect may arise if a bank becomes known for making risky investment choices and is consequently avoided by depositors. Second, shocks, observable or non-observable, common to both assets and liabilities of a bank, if omitted, can cause a bias in the estimate of the effect of leverage on risk taking. This, in particular, is an issue that will arise whenever one measures risk as realised risk in the portfolio, as is common in the previous literature.

We conduct our empirical analysis by using instrumental variable estimation. We first address the issue of reverse causality by making use of bank office level data on deposits for US banks and geographically granular unemployment data. We argue that local unemployment rates are exogenous to the risk taking of a bank as a whole, and construct an instrument based on bank's exposure to deposit supply shocks caused by changes in local unemployment rates, to use in our final regression of leverage on risk. However, as is often the case in the empirical literature, if risk taking is approximated by using a risk measure of existing portfolios, the issue of omitted shocks, which may be common to both assets and liabilities, remains. To address this, we construct a measure of risk taking based solely on newly issued mortgage loans, by considering the universe of mortgage loan applications from 1999 to 2016. We argue that, while the existing portfolio of a bank can be affected by geographical area specific shocks which affect deposits and leverage at the same time, newly issued loans are chosen by banks after the shocks have realized. Hence, the riskiness of newly issued loans is a choice by the bank, unaffected by local area shocks.

Our results confirm that limited liability induces banks to take on more risk after an exogenous increase in leverage, with more leveraged banks being significantly more likely to issue risky mortgage loans. We find that decreasing a bank's leverage ratio by 1 percentage point (which corresponds to an increase in leverage), will lead to this bank originating a loan of 'average' risk with a 3.8% higher probability. In a second estimation we find that this translates into a 1% increase in the predicted median probability of default of loans issued by this bank.

The remainder of this paper is structured as follows: section 1.2 provides a review of the relevant literature, sections 1.3 explains the methodology and the data. Section 1.4 presents the results, while section 1.5 discusses their robustness with respect to the measure of risk taking. Finally, section 1.6 concludes and discusses the policy implications of the findings.

1.2 Literature Review

There are several theoretical papers on how banks' funding structure should impact their risk taking. Jensen and Meckling (1976) show that for a firm the decision to take on debt is equivalent to buying a call option from its creditors. When the debt is due, they can either choose to redeem the bond (buy back the firm, so to speak) or not to. Since the value of such an option is increasing in volatility (see for example Black and Scholes (1973)), firms have an interest to take on higher amount of risk than without debt financing. On the other hand, as Laeven and Levine (2009) point out, requiring banks to hold more capital may not necessarily reduce these incentives if this capital is raised by issuing equity to new shareholders. Simply adding more shareholders with the same incentives may not actually alleviate the issue. Instead, shareholders may decide to make up for the higher cost of capital by taking on even more risky projects in the spirit of Koehn and Santomero (1980). Finally, Diamond and Rajan (2001) show that demand deposits can have a disciplining effect on banks. Our paper contributes to the literature by providing a well-identified answer about the causal effect of leverage on risk taking.

In the empirical literature, there are two ways that previous work has addressed the question of how leverage impacts bank risk taking. The first strain of papers seeks to provide an answer through a direct regression of a measure of leverage on some measure of risk. Altunbas et al. (2007) aim to identify the relationship between leverage and risk by means of a seemingly unrelated regression design that relates changes in capital and risk. They use loan-loss provisions as a proxy for the risk taken on by the bank and find that in their whole sample, banks with a higher equity to asset ratio will take on more risk, while the relationship is negative for the most efficient banks in the sample. Jacques and Nigro (1997) employ an approach that uses a regulatory pressure variable, a measure of how far a bank's equity holdings are from the regulatory threshold, to identify the effect of leverage on risk, but can not refute the null hypothesis that leverage has no effect on risk taking. Koudstaal and van Wijnbergen (2012) aim to identify the effect of leverage on risk by regressing the standard deviation of returns on assets on lagged leverage while controlling for market volatility. They find that higher leverage leads to less risk taking, but that this result is entirely driven by low leverage banks. Highly leveraged banks, they find, do not react to changes in leverage. Similarly to the above paper, Shrieves and Dahl (1992) find that banks take on more risk when there is a positive shock to capital by using a simultaneous regression framework. As we will argue in our methodological section below, all these papers have shortcomings in two ways: first, they fail to provide a convincing identification in the sense that leverage cannot be seen as exogenous in any of the above models. Second, as all the papers employ some measure of portfolio risk, they consider a rather noisy measure of risk taking which is also impacted by market conditions, which may in turn be impacting banks leverage decisions.

The second strain of papers in the literature attempts to provide exogenous variation to leverage by evaluating the effect of policies that impact banks ability to leverage out. Laeven and Levine (2009) find that requiring banks with an owner that holds a significant voting share to hold more capital has the effect of reducing risk taking, while the opposite is true for widely held banks. Acosta Smith et al. (2017) find that the introduction of the leverage ratio requirements in the Basel III framework did cause banks to increase their capital holdings and reduce their risk taking. Finally, Ashraf et al. (2016) show that the introduction of risk weighted capital standards led to a reduction of bank portfolio risk in Pakistan. We add to the empirical literature by providing clean identification of the causal effect of leverage on risk taking, both by adding a new instrument for leverage and by using a measure of risk taking (a banks decision to issue certain loans) that is much less likely to be subject to outside influences and previous choices than the portfolio based measure currently used in the literature. While these papers suffer from the endogeneity associated with leverage to a smaller degree, we believe that our identification is superior as we do not need to rely on the assumption that banks did not react to news or rumors of potential policy changes prior to the implementation of the reform. Additionally, all of these papers rely once more on portfolio based measures of risk, while the present work employs a much more direct measure of risk taking.

In parallel work to this, Ohlrogge (2017) uses an approach that is similar to the one taken in this paper and comes to results that confirm our analysis.

Methodologically, our paper is related to a paper by Bartik (1993) that employs local industry shares to identify the impact of labour supply on wages. This approach has been recently formalized and discussed by Goldsmith-Pinkham et al. (2017).

1.3 Methodology

1.3.1 Overview

In this section we explain the endogeneity issues that plague the analysis of leverage and risk taking, as well as our methodology to tackle them. Before diving deeper into those issues, some definitions will prove useful in the following discussion. First, we will use the term **risk taking (behaviour)** as an act of making new investments (issuing new loans) with different degree of riskiness attached to them. This is the subject of our analysis. It is important to distinguish it from the term **riskiness of the portfolio**, which is defined by the riskiness attached to loans which have been issued in the past. Variation in riskiness of the portfolio can be caused by both risk taking behaviour and by current and past shocks absorbed by the portfolio. Although the riskiness of the portfolio is often used to proxy risk taking behaviour in the literature, the distinction will be important in understanding the issue of endogeneity and our identification.

Most commonly, the literature defines an increase in leverage as an increase in debt financing relative to equity financing by a bank. Basel III however defines a leverage ratio as core equity relative to total assets. For the purpose of easy application to the most recent regulation, we use the latter as a measure of leverage in our econometric analysis. Thus, an *increase* in leverage due to an increase in debt financing corresponds to a *decrease* in the leverage ratio.

We identify two sources of endogeneity which prevent a causal interpretation of a simple regression of leverage on some commonly used measure of riskiness of the portfolio.

- Simultaneity/reverse causality: For a given level of equity, more deposits may incentivise banks to undertake riskier investments, but the demand and supply of deposits are also affected by the riskiness of the portfolio and a bank's risk taking behavior.
- Omitted shocks common to the portfolio and the deposits.

To tackle the first source of endogeneity we build an instrument for leverage that is exogenous to a bank's risk choices. To this end, we use data on deposits at the office level provided by the FDIC. This detailed geographical information on deposits enables us to compute bank exposure to local unemployment variation, which we use as an instrument providing us with variation in leverage exogenous to bank behaviour. This measure, however, can still be endogenous if it relies on the existing portfolio. If some exogenous shocks to economic activity occur, they are likely to not only affect deposits (and leverage) through unemployment but also through the riskiness of the existing portfolio. To tackle this issue, we construct a measure of risk taking based on the new issuance of mortgage loans. We argue that while riskiness of the portfolio may be affected by some shocks which are common to both deposits and assets, the riskiness of new issuances are a choice for banks.

Following the procedure described above, we conduct our empirical analysis by, first, constructing an instrument to assure variation in leverage that is independent of bank risk taking and constructing a measure of risk taking based on newly issued mortgage loans, which is independent of local area shocks. We then regress our instrumented leverage ratio on the riskiness of new issuances in order to estimate the causal effect of leverage on bank risk taking. The methodology is explained in more detail after a discussion of the data employed.

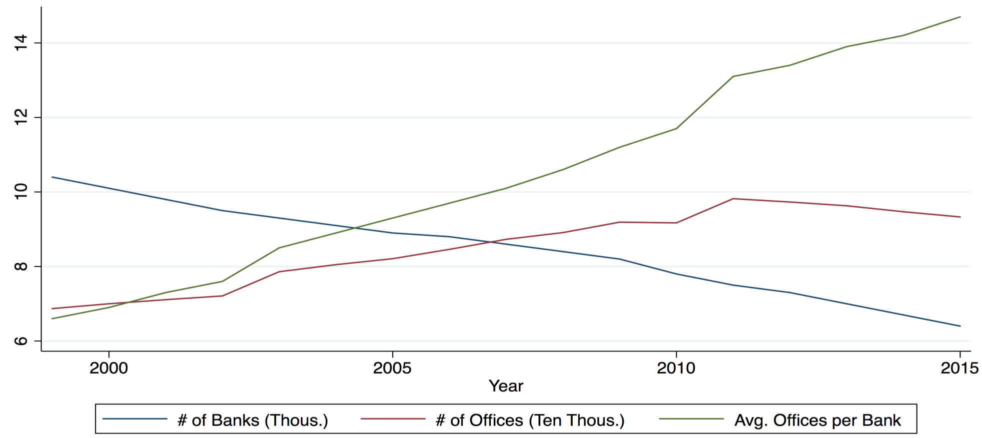
1.3.2 Data

There are four main sources of data that we use in the analysis. The bank-office level deposit data from the FDIC, and the local unemployment data from the Bureau of Labour Statistics are used in constructing the instruments. We use the Home Mortgage Disclosure Act data on the universe of mortgage applications from the FFIEC to construct a measure of risk taking. Finally, we add balance sheet data from the FDIC to calculate the leverage ratio in the IV estimation of the effect of leverage on risk taking.

Summary of deposits (FDIC) Summary of deposits data is annual data on the level of deposits at the bank office level. For every office, for every bank operating in the US and insured by the FDIC, the amount of deposits as of June 31st is reported. Along with the deposit level, the data contains detailed geographic and demographic information for every office as well as an identifier for the owning bank. This identifier is then used to merge this data with balance sheet data also provided by the FDIC.

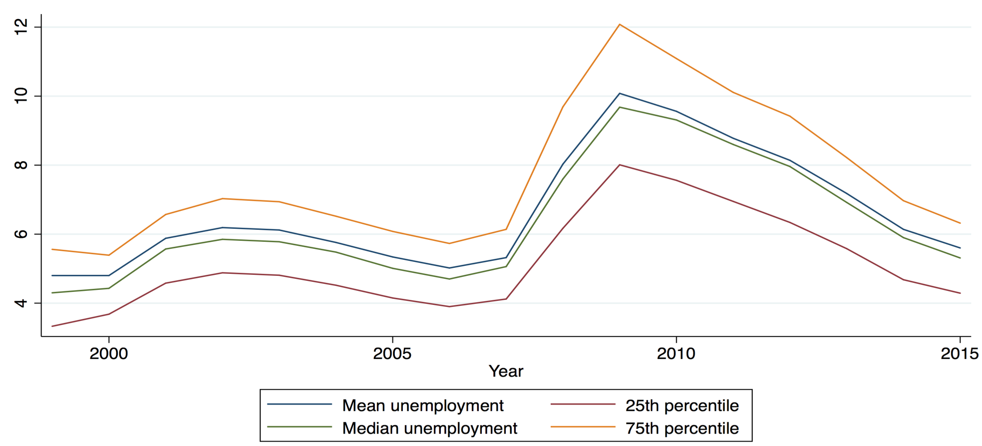
For the purpose of our analysis, the data for every bank is collapsed at a relevant geographical area level. We will be using the Core Based Statistical Areas. A Core Based Statistical Area (CBSA) consists of one or more counties (or equivalents) anchored by an urban center of at least 10,000 people plus adjacent counties that are socioeconomically tied to the urban center by commuting. Not all counties are a part of a CBSA. Around 10% of all observations come from counties which are not part of any CBSAs adding up to around 5% of all deposits. We aggregate these counties at the state level into CBSA equivalents and brand them as *rural state areas*. Figure 1.1 presents some descriptive statistics from the Summary of Deposits data. Two features of the data are noteworthy. First, over our sample period of 1999 to 2015, there is a significant consolidation in the banking market, with the number of banks decreasing by about 40%, while the number of offices per bank more than double. We take care that this development does not impact our analysis by removing an office sold by bank i to bank j in year t from the sample in years t and $t-1$, so that the change in deposits between $t-1$ and t for neither bank i nor bank j is affected by the transfer of ownership of that office.

FIGURE 1.1: BANK OFFICES DATA



Local area unemployment statistics (BLS) Local area unemployment statistics provide monthly data on unemployment at the county level. Since the relevant geographical area in constructing the instruments is the CBSA, we aggregate the statistics to the CBSA level at yearly frequency. Figure 1.2 presents the descriptive statistics of unemployment rate of the CBSAs in the US within our sample period. This graph shows that there is indeed significant heterogeneity between CBSAs, when it comes to unemployment, providing variation to exploit when constructing our instrument. As we show later, this heterogeneity in unemployment rates will have heterogenous effects on banks, which are differentiated by the geographic composition of their deposit holdings. Finally, the first stage regression of our instrumental variable approach shows that this variation is strongly correlated with leverage at the bank level.

FIGURE 1.2: CBSA UNEMPLOYMENT STATISTICS



Home mortgage disclosure act data (FFIEC) The Home Mortgage Disclosure Act (HMDA) mandates that banks above a set threshold of assets issue detailed reports on their mortgage applications, lending and purchases. The reporting is done through the Loan Application Registries (LAR) and includes all mortgage loan applications within a year. Moreover, the registries contain some characteristics of the applicant and potential co-applicant (ethnicity, race, gender, income), as well as characteristics of the loan (amount, type, purpose, rate spread for some, occupancy), the property (type, census tract, etc.), the census tract in which the property is located (income relative to the MSA, minority population, number of housing units, etc.), as well as the action taken by the bank (origination, denial and its reason, sale to an institution like Freddie Mac).

Our construction of the measure of risk taking loosely follows DellAriccia et al. (2012) and relies on the loan to income ratio. The loan to income ratio is computed as the total loan amount in the application over the total gross annual income an institution relied upon in making the credit decision[1]. To add to the methodology on a measure of risk taking we also use the data on origination. We define origination as an application which has been accepted and then either originated or refused by the applicant, a purchase of a loan, or a preapproved request. We define a non-origination as an application denied by the bank or a denied prerequest. We ignore all applications withdrawn by the applicants or applications closed for incompleteness.

The LAR data reports all applications, accepted or rejected. Table 1.1 provides the statistics on the origination ratio between 2004 and 2012. The share of originated loan applications (see column (1)) decreased from 74% in 2004 to 67% in 2007. In 2009, the origination ratio increased sharply and then gradually increased to 80% in 2012. The sharp increase reflects the crisis, which has decreased the demand for loans and forced the worse potential borrowers out of the market. The remaining pool was of higher quality which increased banks willingness to lend to remaining applicants.

Table 1.1: Reason for denial

Year	Origin. ratio	Reason for denial					
		DtI	Empl. hist.	Cred. hist.	Coll.	Dwnpay.	Other
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
2004	.744	.131	.010	.299	.113	.0154	.432
2005	.729	.122	.0111	.267	.122	.012	.466
2006	.715	.150	.0129	.284	.154	.017	.381
2007	.677	.173	.0123	.272	.193	.017	.332
2008	.681	.205	.0114	.265	.250	.018	.249
2009	.767	.227	.0129	.209	.310	.020	.220
2010	.776	.222	.0134	.202	.252	.021	.288
2011	.766	.214	.013	.223	.241	.021	.285
2012	.794	.213	.013	.233	.218	.024	.301

Table 1.1 also reports the shares of prevailing reasons for rejecting a loan. Insufficient collateral (column (5), high debt-to-income (column (2)) and poor credit history (column (4)) explain the bulk of the rejection decisions. The effect of the crisis is evident in the spike of the share of rejections due to insufficient collateral in 2009 when house prices collapsed. We take into account this crisis effect by including time fixed effects in our estimation of the risk measures.

On top of the loan application data, the LAR reports also the information about the loans purchased by banks. Tables 1.2 provides the statistics about the characteristics of all applications, the accepted applications, the rejected applications and the purchased loans. The table shows that accepted loans have, on average, a lower loan-to-income ratio than rejected loans.

[1] Gross annual income is not registered in HMDA due to four possible reasons: (i.) multi-family dwellings, (ii.) income was not registered in the loan purchase documentation, (iii.) loans to bank employees, (iv.) loans to non natural persons. These cases are excluded from the estimation as described in the methodology section

Table 1.2: Loan-to-Income ratio

	All		Accepted		Rejected		Purchased	
	mean	p50	mean	p50	mean	p50	mean	p50
2004	2.274	2.083	2.227	2.103	2.408	2.000	2.502	2.390
2005	2.281	2.098	2.217	2.106	2.452	2.077	2.480	2.400
2006	2.188	1.974	2.109	1.951	2.385	2.038	2.322	2.233
2008	2.304	2.098	2.184	2.059	2.553	2.200	2.547	2.451
2008	2.433	2.209	2.311	2.174	2.687	2.300	2.735	2.607
2009	2.573	2.243	2.420	2.211	3.060	2.372	2.539	2.427
2010	2.430	2.139	2.309	2.115	2.846	2.244	2.673	2.540
2011	2.349	2.023	2.222	2.000	2.761	2.083	2.587	2.435
2012	2.384	2.054	2.291	2.051	2.735	2.065	2.573	2.409

Balance sheet data (FDIC) To construct the leverage measure of banks, we use balance sheet data provided by the FDIC. The data is available at quarterly level and includes income statements as well as several performance ratios.

1.3.3 Exogenous Variation in Deposits: Two Instruments

The aim here is to construct an instrument to assure that the variation in leverage is independent of risk taking. We do so in two ways: i) estimating shocks to local deposits caused by unemployment changes which we then aggregate for each bank by computing the weighted average of these local shocks; ii) or directly computing the weighted average of unemployment changes and using this as an instrument for leverage.

To this end we use data on deposits at the bank-office level, administered by the Federal Deposit Insurance Corporation (FDIC) and the Local Area Unemployment Statistics administered by the Bureau of Labour Statistics. The first dataset contains yearly information on the level of deposits for all offices of all banks insured by the FDIC, together with the demographic information on the office and the bank which owns it. The second dataset provides monthly unemployment figures at county level. The relevant geographical definition in our analysis is the Core Based Statistical Area.

The rationale behind these instruments for bank level deposit growth rates follows closely Bartik (1993), whose approach has been extensively analysed in Goldsmith-Pinkham et al. (2017). The standard idea behind this approach is that when one is interested in a parameter, say, the elasticity of labour supply, using changes in wages and employment growth rates at local level, one should be concerned with the endogeneity of local employment growth. To solve this issue, the Bartik approach suggests to define an instrument as the local employment growth predicted by interacting local industry employment shares with national industry employment growth rates.

In our setting we follow a similar logic but we apply it to a different level of granularity of the data. Our potentially endogenous object is the deposit growth rate of banks. We therefore build as an instrument the predicted change in deposits for a bank in a given period as the interaction between the bank's geographical area deposit share and the change in deposits in the geographical area.

We do so in two different ways to avoid any further endogeneity concern or feedback loop between bank and area level deposit changes: i) we predict the change in deposits in a geographical area in a given period based on the change of local unemployment in that period and use this fitted value as our instrument; ii) we use the change in unemployment in the geographical area directly (not using it to predict deposits) as the instrument.

Before discussing the two approaches in further detail, key differences between our strategy and the standard Bartik instruments should be highlighted. As mentioned above, the literature employs this approach to solve endogeneity problems. In contrast, we use the geographical area shares as a mean of aggregation, not as a solution to endogeneity per se. We adopt as instrumental variables for the leverage the "relevant" changes in local unemployment or deposit supply, where "relevant" is to be read as weighted by geographical area deposit composition. The adoption of this specific aggregation strategy serves two distinct purposes: first and foremost is an appealing way of aggregating geographical area specific changes to the bank level; second it eliminates any further endogeneity concerns. The two approaches are formalized below.

Instrumenting deposits : In building the first instrument we regress the growth rate of total deposits in a geographical area on the change in the local unemployment rate. We brand the fitted values from this model at the geographical area level as shocks to deposit supply at the geographical area level. For each bank in each year we then compute the exposure to this variation in deposit supply as a weighted average of these shocks using the deposits each bank holds in a particular area as weights. This implies the following procedure,

$$\Delta dep_{i,t} = \alpha_0 + \gamma_i + \eta_t + \beta \Delta unemp_{i,t} + \epsilon_{i,t} \tag{1.1}$$

where $\Delta dep_{i,t}$ denotes the growth rate of deposits in a geographical area i in period t, γ_i and η_t denote geographical area and time fixed effects, and $\Delta unemp_{i,t}$ denotes a change in unemployment rate in geographical area i in period t. We call the fitted values from the model above local deposit supply shocks. To compute the exposure of a particular bank in a particular period to these shocks, which will serve as an instrument for leverage in our final estimation, we compute the weighted average of these shocks for every bank, where then we use the deposit this particular bank holds in different areas as weights. For bank b, operating in areas $i = 1..I$, this implies:

$$\Delta \hat{dep}_{b,t} = \frac{\sum_{i=1}^{I} dep_{b,i,t} \Delta \hat{dep}_{i,t}}{\sum_{i=1}^{I} dep_{b,i,t}} \tag{1.2}$$

where $dep_{b,i,t}$ denotes the deposits bank b holds in geographical area i in period t. We use the measure $\Delta \hat{dep}_{b,t}$ as one of the possible instruments in the final estimation of the effect of leverage on risk taking.

Table 1.3.3 presents the estimation results for equation 1.1. Results, as expected, prove a negative and highly significant effect of changes in unemployment on deposit growth rates at the CBSA level. An increase in unemployment change in a CBSA by one percentage point decreases the deposit growth rate in that area by 0.43 percentage points after controlling for the CBSA and year fixed effects.

Table 1.3: First preliminary stage

	(1) Δln(deposits)
Δ unemp	-0.432****
	(0.117)
constant	0.0310****
Time FE	YES
CBSA FE	YES
N	21450
R^2	0.014

Standard errors in parentheses
** $p < 0.05$, *** $p < 0.01$, **** $p < 0.001$

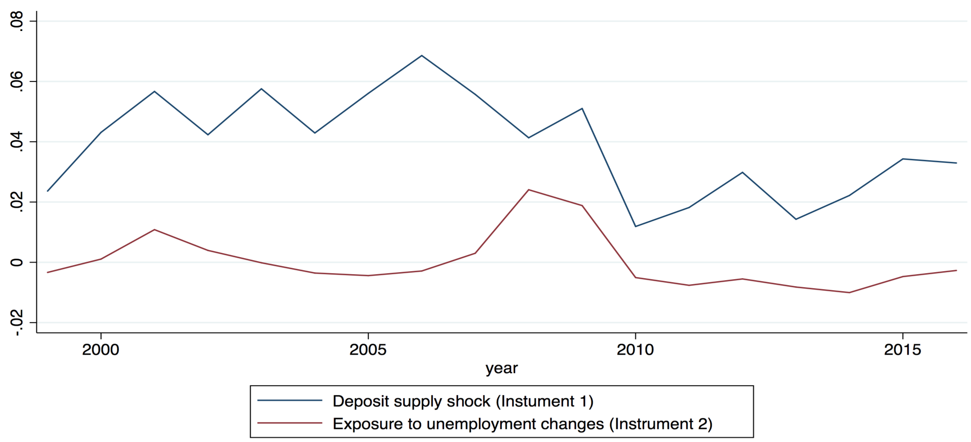
Direct local unemployment exposure: In building the alternative instrument we directly estimate the exposure of each bank to changes in local unemployment rates, using, as before, the deposits a bank holds as weights. For bank b, operating in areas $i = 1..I$, this exposure, $\Delta exp_{b,t}$, is given by:

$$\Delta exp_{b,t} = \frac{\sum_{i=1}^{I} dep_{b,i,t} \Delta unemp_{i,t}}{\sum_{i=1}^{I} dep_{b,i,t}} \tag{1.3}$$

where, as before, $dep_{b,i,t}$ denotes the deposits bank j holds in geographical area i in period t, and $\Delta unemp_{i,t}$ denotes a change in unemployment rate in geographical area i in period t. We use $\Delta exp_{b,t}$ as the second instrument in the final estimation of the effect of leverage on risk taking.

Figure 1.3 plots the mean of the bank level deposit supply shock, $\hat{dep}_{j,t}$ across time, and the mean bank level exposure to unemployment, $\Delta exp_{b,t}$. The figure reveals a sharp drop in deposit supply coinciding with a spike in unemployment across the US.

Figure 1.3: Mean bank exposure to local unemployment changes and deposit supply shocks



The two instruments exploit the same variation of changes in unemployment at the local level. They are not numerically equivalent due to different specifications of the fixed effects. It is also worth noting that the second instrument does not require any estimation since it is only built through aggregation of local areas changes at the bank level. This is relevant because one may be concerned that our first instrument may suffer from generated regressor problems. As we will show later we obtain fairly similar results with the two instruments.

1.3.4 A Measure of Risk Taking

Remaining endogeneity The procedure explained above describes constructing a measure of an exogenous change in deposits and a measure of an exposure of banks to changes in local unemployment rates. Both these measures are exogenous to risk taking, but not exogenous to a measure of riskiness of the portfolio. To exemplify the issue, consider a shock to deposits in a certain area as estimated in the previous section. Such a shock is likely to impact the income of depositors. It cannot, however, be excluded to have impacted also the borrowers, private or corporate, in an area, which may or may not have borrowed from the banks operating in that areas. Any measure of risk, which is based on the performance of the existing portfolio might be subject to this sort of residual endogeneity.

New issuances of loans are not subject to this endogeneity concern since new issuances can only be affected by the existing pool of potential loans. A geographical area shock can affect the existing local pool of borrowers, while it does not affect the entire pool of potential borrowers. New issuance of a loan is a choice for a bank and the riskiness of new issuances proxies risk taking behaviour.

Creating a measure of risk taking To this end we construct a measure of risk taking based on the issuances of new mortgage loans based on the Home Mortgage Disclosure Act (HMDA) dataset, administered by the Federal Financial Institutions Examination Council (FFIEC). It is a yearly dataset on the population of mortgage applications to banks and other mortgage lenders with detailed information on the borrower and loan characteristics. We take the riskiness of new mortgage lending as representative of risk taking on the entire portfolio.

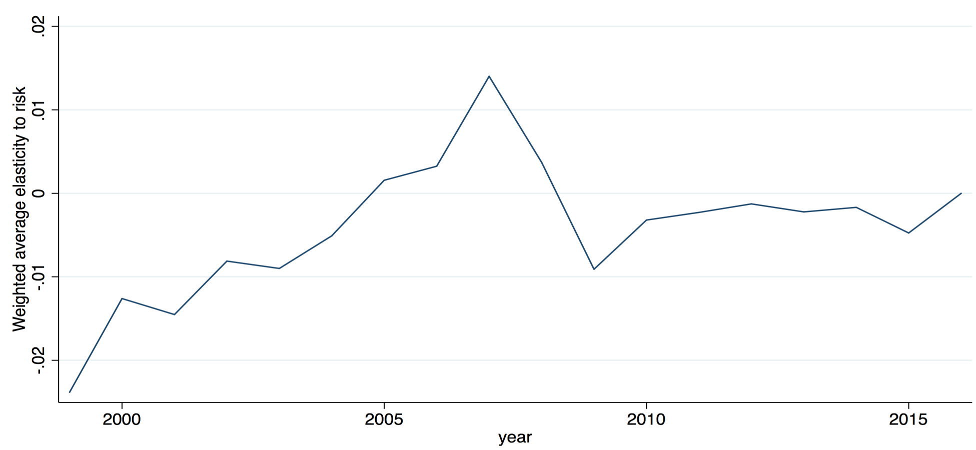
To construct a measure of risk taking behaviour by banks, we estimate the responsiveness of loan issuance of each bank in each year to riskiness of the borrower and the loan. As a measure of riskiness of the loan and the borrower we use the Loan-to-Income (LtI) ratio computed from the HMDA dataset for every loan application. This follows loosely DellAriccia et al. (2012), where LtI is used directly as a measure of risk in their analysis of lending standards. This methodology implies the following model[2,3].

$$Origin_{t,b,j} = \gamma_t^0 + \gamma_{t,b}^1 LtI_{t,b,j} + \epsilon_{t,b,j} \tag{1.4}$$

where $Origin_{t,b,j}$ denotes a binary loan origination variable which takes the value $Origin_{t,b,j} = 1$ if the application in period t to a bank b by a borrower j is accepted and loan is originated, and takes the value $Origin_{t,b,j} = 0$ if the application is rejected and the loan is not originated. γ_t^0 captures the effect of the macroeconomic situation in period t for all banks, such as market conditions and regulation, which may cause banks to have differing appetites for risk over time. Finally, for every bank b in every period t we also obtain an estimate of the risk responsiveness $\hat{\gamma}_{t,b}^1$ based on Loan-to-Income of all applicants j, which serves as a measure of risk taking behavior by banks.

Figure 1.4 plots the risk measure for the banks included in the analysis over the years. The distribution has a mean of $-.0039622$.

Figure 1.4: Responsiveness of origination to risk



[2]In order for γ_t^0 to capture the macroeconomic conditions affecting the origination choices, we estimate the model for all banks reporting to the HMDA dataset but only use the $\gamma_{t,i}^1$ for banks included in the final regressions. This implies including all the loan applications in the HMDA reporting in the estimations. The number varies between 17 million applications and 40 million application which constrains us to estimating the model as a linear probability model.

[3]This measure is joint work with Lopez-Quiles and Petricek (2018)

1.3.5 IV Estimation of the Effect of Leverage on Risk Taking

In estimating the effect of leverage on risk taking we use the two instruments, explained in detail above. As argued before the instrumented deposits and the direct measure of exposure to local unemployment shocks are exogenous to risk taking. The instruments allow us to estimate two effects: (i.) the effect of the two instruments on leverage, and (ii.)the effect of leverage on risk taking.

More specifically we run the following two specifications:

$$\hat{\gamma}_{b,t} = \beta_0 + \beta_1 lev_{b,t} + \eta_b + \delta_t + \epsilon_{bt} \tag{1.5}$$

Where lev_{bt} is the endogenous variable, measured as the leverage ratio, η_b are bank fixed effects and δ_t are time fixed effects which are included to control for different baseline risk preferences of banks as well as a regulatory environment that may change over time. This equation is then estimated by IV, where the endogenous variable lev_{bt} is instrumented with one of the two instruments: either $\Delta \widehat{dep}_{bt}$ or Δexp_{bt}, depending on the model. It is also estimated by OLS, in order to compare the coefficients of interest.

The results of the estimations are presented and discussed in the next section.

1.4 Results

Table 1.4 presents the results of the estimations for both instruments. Column (1) shows the biased OLS estimate of regressing the risk taking measure on leverage ratio. Columns (2) and (3) present the results using the deposit supply shocks as an instrument, while columns (4) and (5) present the results employing the exposure to changes in unemployment. Columns (2) and (4) show the first stage of the two IV regressions, while Columns (3) and (5) show the second stage. Both sets of results are consistent in terms of sign, so we will focus on the latter in explaining them. All standard errors are clustered at the bank level.

First we find that the naive OLS approach severely underestimates the effect of leverage on risk taking behaviour, the result being close to zero and statistically insignificant. This bias may go some way towards explaining findings in the previous literature that leverage leads to less risk taking, or has no effect (see for example Altunbas et al. (2007) and Jacques and Nigro (1997)).

All our results provide evidence for a positive effect of leverage on banks' risk taking due to limited liabilities[4].

We also investigate the possibility of nonlinear effects of leverage on risk taking by running our IV regression through 2SLS and interacting the predicted endogenous variable with dummy variables denoting different deciles of the leverage distribution. We find no significant pattern in the estimation and that most coefficient on these dummies are not statistically significant. The takeaway of this analysis is that the effect of leverage in our data does not vary significantly along the distribution of leverage.

[4]Since leverage is measured as leverage ratio (core capital over total average assets), a negative sign implies that an increase in leverage (a decrease in leverage ratio) increases risk taking by banks

Table 1.4: Risk Measure

	(1) Risk	(2) Leverage	(3) Risk	(4) Leverage	(5) Risk
Leverage	-0.0000902		-0.0118**		-0.00564**
	(0.0000801)		(0.00557)		(0.00240)
IV Δ Unemployment		5.462****			
		(1.287)			
IV Δ Deposits				-25.12****	
				(2.817)	
Non-Current Loans		-0.163****	-0.00255***	-0.164****	-0.00155****
		(0.00404)	(0.000913)	(0.00403)	(0.000400)
Constant	0.0461****	9.776****	0.160***	10.35****	0.101****
	(0.000977)	(0.0290)	(0.0544)	(0.0713)	(0.0234)
Time FE	Yes	Yes	Yes	Yes	Yes
N	65811	65660	65660	65660	65660
R^2	0.118	0.056		0.057	
$F(instr.excl.)$		17.72		79.00	

Standard errors in parentheses
** $p < 0.05$, *** $p < 0.01$, **** $p < 0.001$

Quantitatively our results state that a one point increase in the leverage ratio[5] generates an .0118 decrease in our risk taking measure. The mean of the risk measure in our data is .038, which implies that a one point increase in the leverage ratio produces a 31% decrease risk taking when compared to the average.

For a specific example, assume that two banks are identical except for their leverage ratios, which differ by one percentage point. Assume that they receive the same application for a mortgage loan with average Loan-to-Income. Our estimates suggest that this application has an expected probability of being originated in the bank with the higher leverage (i.e. lower leverage ratio) of $\gamma_0 + \gamma_i LTI$ whereas the expected probability of origination for the less leveraged bank is $\gamma_0 + (\gamma_i - .0118)LTI$. Evaluating these probabilities at the average of our estimates and at the average loan-to-income we obtain that the more leveraged bank has a 3.1% higher probability of originating the loan.

Note also that this wedge between the probabilities of acceptance increases with the loan-to-income ratio. Meaning that the higher the loan-to-income of the applicant the larger the difference in expected acceptance probabilities between the more and less leveraged bank.

Our results have policy relevant implications in terms of the aggregate level of risk in the banking system. The estimations show that more levered banks are more likely to take on riskier projects due to limited liability incentives which implies that curbing leverage has the added benefit of reducing banks' risk taking, thereby producing a more resilient banking system.

1.5 Robustness

In our main analysis we use as the outcome a risk measure based directly on banks' lending decisions. The novelty of this measure lies in the fact that it captures the responsiveness of origination behaviour to a proxy of risk, namely the loan to income ratio. One could however be concerned that

[5] Leverage ratio is defined as core capital divided by assets. Thus an *increase* in the leverage ratio is associated with a *decrease* in bank leverage.

LtI being our only proxy for risk in our estimation the γ parameter may be capturing correlations of LtI with unobserved loan characteristics. In order to address this potential problem redo the above analysis with a second measure for bank risk taking.

We resort to a further data source (Consumer Financial Protection Bureau) to obtain mortgage delinquency rates at the county level. This data covers 470 counties for the period 2009-2015. The sample is representative and covers approximately 85% of all mortgage lending in the HMDA data. We use the 90 days delinquency rate for closed-end, 1-4 family residential mortgages. The full list of variables and sources is in Table 1.6 in the Appendix.

The idea of this procedure is that we can observe the delinquency rate at the county level and we obtain a number of predictors with the same level of disaggregation. Once we have established a prediction model for our outcome we can predict the delinquency rate at the loan level by using more disaggregated data from the HMDA.

From the county data we build a 3 years ahead average delinquency rate at the county level, which will be used as the outcome of our prediction model. We obtain a number of explanatory variables from the American Community Survey, the Internal Revenue Service and the Bureau of Labour Statistics.

Since the goal is to be able to predict the delinquency rate at the loan level we run the model including two sets of explanatory variables: i) regressors that we observe both at the county and at the loan level; 2) regressors we only observe at the county level.

In order to find the best prediction model we estimate approximately 1000 models including combinations of the predictors. For each model we split our sample keeping 75% of the counties to estimate the model and 25% as out of sample fit and we estimate it 10 times. We evaluate the models by the out of sample Mean Squared Prediction Error and pick the one with the lowest MSPE. All models are estimated with and without time fixed effects.

More formally we estimate:

$$DR_{i,t} = \beta_0 + \beta_1 X^1_{i,t} + \beta_2 X^2_{i,t} + \beta_3 X^1_{i,j,t} X^2_{i,t} + \delta_t + \epsilon_{i,t} \tag{1.6}$$

Where i denotes the county and X^1 is a set of predictors that is aggregated at the county level from the loan level data, whereas X^2 is purely county level data. We include the interaction term between the loan aggregated and the county level regressors to account for the covariances between the two and eliminate potential bias when we move to the loan level prediction.

Once we have estimated this model we use the coefficient and produce the following loan level prediction

$$\hat{D}R_{i,j,t} = \hat{\beta}_0 + \hat{\beta}_1 X^1_{i,j,t} + \hat{\beta}_2 X^2_{i,t} + \hat{\beta}_3 X^1_{i,j,t} X^2_{i,t} + \hat{\delta}_t, \tag{1.7}$$

where j denotes a loan. The specification of the prediction model is displayed in Table 1.7 in the Appendix.

This procedure gives us an expected probability of default for all the loans in our sample. In order to use this as an outcome in our final stage we take the median forecasted probability of default for each bank in every year. We estimate this model using the IV discussed in the methodology section. The results of this procedure are displayed in Table 1.5.

Table 1.5: Regression table

	(1) Median PD	(2) Leverage	(3) Median PD	(4) Leverage	(5) Median PD
Leverage	-0.0208**		-1.174***		-1.025***
	(0.00861)		(0.434)		(0.374)
IV Δ Unemployment		6.099****			
		(1.541)			
IV Δ Deposits				-15.22****	
				(3.510)	
Non-Current Loans		-0.125****	-0.122**	-0.126****	-0.104**
		(0.00527)	(0.0550)	(0.00527)	(0.0475)
Constant	2.592****	10.05****	14.26***	10.95****	12.74****
	(0.0884)	(0.0378)	(4.416)	(0.181)	(3.802)
Time FE	Yes	Yes	Yes	Yes	Yes
N	25050	25040	25040	25040	25040
R^2	0.253	0.092		0.092	
$F(instr.excl.)$		37.05		44.04	

Standard errors in parentheses
** $p < 0.05$, *** $p < 0.01$, **** $p < 0.001$

Using this measure for risk taking yields results that are in line with the ones discussed in the previous section. OLS underestimates the effect of leverage on risk taking for this measure as well. Using the IV procedure yields a negative and significant effect of leverage ratio on risk taking. This once again translates into a positive relationship between bank leverage and risk taking. Our results do not differ between the two IV procedures. Using this risk measure, we find that an exogenous one point increase in the leverage ratio of a bank will lead to roughly a one percentage point decrease in the median probability of default of loans issued by that bank.

1.6 Conclusions

This paper addresses the question of the causal effect of changes in leverage on banks' risk taking behaviour. We do so by constructing two instruments to overcome the endogeneity problems resulting from the potential simultaneity and reverse causality between risk decisions and the deposit market conditions. We instrument exogenous changes in leverage by building two instruments: i) one based on the geographical area unemployment changes; ii) one based on the geographical area deposit supply changes. In both cases we aggregate them using the local deposit share of banks.

We then build a new measure of risk taking behaviour based on the responsiveness of origination decisions to a measure of risk of loan applications (loan-to-income). We compute this measure at the bank/year level and use it as our outcome.

Our empirical analysis suggests that exogenous increases in leverage incentivises banks to take on more risk, i.e. to originate loans with riskier characteristics. We also employ a second measure of risk, the median predicted probability of default of originated loans, and find that higher levererage leads to a higher probability of default of issued loans. These results are consistent with a limited liability and moral hazard story put forth by some of the theoretical literature. They are novel in the empirical literature on leverage and risk taking, as previous work has found no relation or a negative relationship between leverage and risk.

These results have relevant policy implications in that they suggest that any measure that would reduce banks' leverage would also decrease incentives to invest in risky assets, thereby considerably reducing systemic risk.

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1.A Variable list

Table 1.6: Data for Prediction Model

VARIABLE	SOURCE	AVAILABILITY
median lti of originated loans for purchases or refinancing	HMDA	loan
median income of borrowers	HMDA	loan level
median borrowed amount	HMDA	loan level
median housing cost	ACS	county level
median housing cost to median income	ACS	county level
median income	ACS	county
share households with a second mortgage	ACS	county level
median value of a housing unit	ACS	county level
average monthly mortgage payment	IRS	ZIP code level
unemployment rate	BLS	county level

Data sources are:
HMDA - home mortgage disclosure act data
ACS - American Community Survey data
IRS - Internal Revenues Service
BLS - Bureau of Labor Statistics

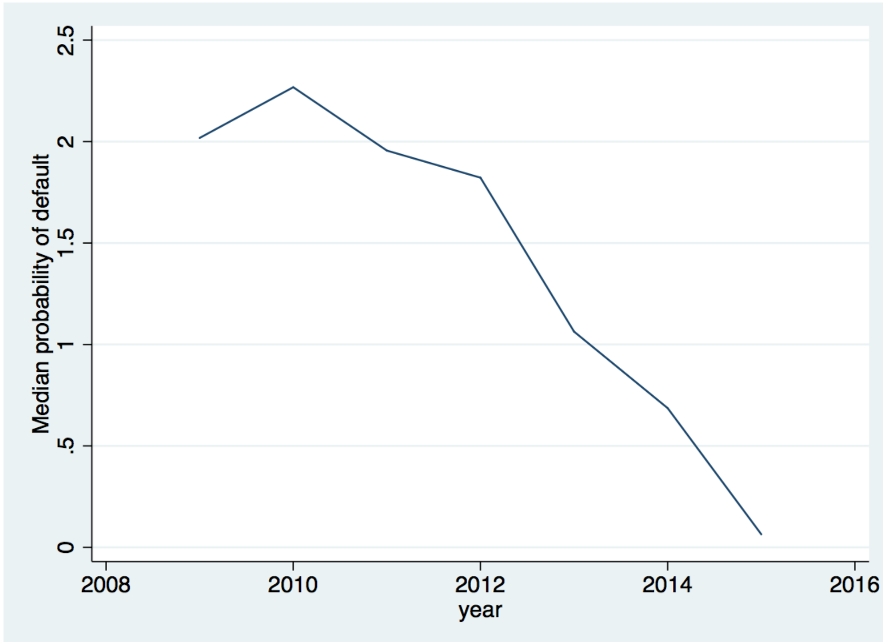
1.B Delinquency rate prediction model

Table 1.7: Prediction Model

	(1) dr3c
Median lti of origin. loans	-0.482
	(0.686)
Median income of borrowers	-0.0621***
	(0.0145)
Median household income	0.0000254
	(0.0000144)
Med. housing cost to household inc.	315.1***
	(71.32)
% of households with 2nd mortg.	0.0417
	(0.0343)
Average mortgage payments	0.000217*
	(0.000110)
Unemployment	0.0000428***
	(0.00000523)
Interaction terms	YES
State fixed effects	YES
Constant	-1.223
	(1.485)
N	1606
R^2	0.793
Adj. R^2	0.783

Standard errors in parentheses
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Figure 1.5: Median predicted probability of default; (average across banks in the final estimation sample)



Chapter 2

Deposit Insurance and Bank Risk Taking

joint with Carolina López-Quiles

2.1 Introduction

Deposit insurance is one of the standard measures to ensure stability of the financial system. By assuring the depositors that their savings will be available to them when their liquidity need arises, they do not react to information or speculation about bank liquidity by withdrawing their deposits. This allows the financial system to weather periods of tight liquidity and maintain a steady flow of lending to households and firms.

In the light of high contingent liabilities for the government, the savings and loan crisis of the 1980s and 1990s and, more recently, the 2000s financial crisis, have spurred a debate about the costs and benefits of providing deposit insurance.

Theory provides two important results regarding the benefits of deposit insurance. On the one hand, as shown in the seminal work by Diamond and Dybvig (1983), deposit insurance eliminates the self-fulfilling equilibrium where depositors find it optimal to withdraw their deposits in the belief that other depositors will do the same. This leaves only depositors with liquidity needs to withdraw their deposits. On the other hand, as shown among others by Cooper and Ross (2002), deposit insurance induces a problem of moral hazard. If no deposit insurance is in place, a bank making riskier investments incurs a cost through compensating the depositors with higher deposit rates. If depositors are insured against potential losses, they will increase their supply of deposits, abstain from demanding a premium for the riskier investments undertaken by the banks, and will be less careful in monitoring their bank. This allows a bank to take on riskier investments.

Empirical literature has thoroughly covered the effect of deposit insurance on risk taking and the extent of market discipline. Focusing on cross country comparison Demirguc-Kunt and Huizinga (1999) find that explicit deposit insurance reduces required deposit interest rates at a cost of reduced market discipline. Similarly, Demirguc-Kunt and Detragiache (2002), find that explicit deposit insurance increases the likelihood of a banking crisis, especially in an institutionally weak environment. Furthermore, the adverse affect is strengthened by the extensiveness of the insurance coverage. Nier and Baumann (2006) focus on the effect of government safety nets like deposit insurance on capital buffers. Their results suggest that safety nets lower the capital buffers, an indication of an increased solvency risk, and that competition among banks dampens this effect. More recently, Anginer et al.

(2014a) estimate the effect of deposit insurance on bank risk, measured by the bank Z-score, and on systemic fragility measured as the marginal expected shortfall (following Acharya et al. (2012)). Their results indicate that deposit insurance increased bank risk and systemic fragility in the years leading up to the global financial crisis. However, systemic fragility was reduced due to deposit insurance during the financial crisis. Focusing instead on a forward looking risk measure (the CDS spread) but still applying a cross country comparison, Liu et al. (2016) find that banks in countries with explicit deposit insurance systems tend to have higher CDS spreads.

A positive effect of deposit insurance on measures of risk found in studies above is further confirmed by Boyle et al. (2015) using a conjoint analysis approach. They find that respondents from countries without explicit deposit insurance exhibit greater risk of withdrawing their deposits in a hypothetical scenario of a failure of a competing bank and a tendency to impose higher deposit interest rate, suggesting a presence of market disciplining. Furthermore, in a natural experiment setting based on the introduction of deposit insurance within the Russian banking sector, Chernykh and Cole (2011), find that deposit insurance increased the solvency ratio and the loan-to-assets ratio, which, they argue, indicates a higher solvency risk and credit risk. Similarly, focusing on a with-in country analysis, based on internal loan ratings, Ioannidou and Penas (2010) find that in periods following the introduction the deposit insurance in Bolivia, banks were more likely to initiate riskier loans. They also conclude that this result is due to lower market discipline imposed by large depositors. Finally, using similar variation in deposit insurance as we do in this paper, Lambert et al. (2017), find that following an increase in deposit insurance coverage, banks which have been more affected by that increase become riskier, i.e., their Z-score has decreased.

The literature on the effect of deposit insurance on risk taking, although vast, exhibits some limitations which, in our opinion, may lead to a failure to identify the causal relationship. Firstly, while a focus on cross country comparisons ensures some variation in deposit insurance, such variation cannot exclude endogeneity of bank behaviour and deposit insurance policies. Furthermore, such an analysis neglects other institutional differences between banking markets. Secondly, it is important to distinguish between different measures of riskiness of the bank, such as the Z-score, which are based on the performance of the portfolio, and measures of risk taking. This is mainly due to the fact that economic shocks, which contribute to establishing deposit insurance, can also affect the state of banks' portfolios, inducing endogeneity. Measures of risk based on new investment are not subject to this shortcoming, as they reveal the choice of banks at the moment of investment.

This paper adds to the literature by addressing these sources endogeneity. Firstly, to make sure that deposit insurance policy is not endogenous, we use a natural experiment setting from the US banking regulation. We analyse an episode where a change in the insurance coverage limit affected some banks but not others in an exogenous way (see below). Secondly, we build a bank-level measure of risk taking based on newly issued mortgage loans. These two methodological contributions allow us to estimate a causal effect of deposit insurance on risk taking.

We exploit an increase in the coverage limit of deposit insurance in the US for identification. In the US, all banks have deposit insurance coverage provided by the FDIC (Federal Deposit Insurance Corporation). In addition, state-chartered savings banks in the state of Massachusetts are provided with *unlimited* coverage by a private insurer. Membership to this unlimited insurance scheme is mandatory for all savings banks chartered in Massachusetts since 1934. The unlimited nature of this coverage is key to our identification.

In order to measure risk-taking, we use mortgage application data from the Home Mortgage Disclosure Act. We estimate the propensity to lend for a given level of borrower risk, which we proxy using loan-to-income ratios of each loan application.

Contrary to the literature, we find no significant effect of an increase in deposit insurance coverage on bank risk taking. Banks are not more likely to grant a loan for any given loan-to-income ratio of the borrower after such an increase.

Furthermore, to explore the reason for the absence of a significant effect, we use the same natural experiment setting to test whether an increase in deposit insurance reduces vigilance by depositors as indicated by lower movement in the supply of deposits. Theory suggests that the incentive to increase risk taking stems from a relaxed deposit supply which does not penalise such risk taking. We find that an increase in deposit insurance does not reduce deposit interest rates, indicating a lack of a market-discipline effect. This result confirms prior findings of Schmukler (2001) and rationalises a lack of an effect on risk taking.

This paper is organised as followed: section 2 presents the mortgage and balance sheet data used in the empirical analysis. Section 3 presents the methodology, the natural experiment setting and the construction of the risk taking measure. Section 4 presents the results of the estimation of the effect of an increase in deposit insurance coverage on risk taking. Section 5 presents the results of the estimation of the effect on deposit supply. Section 6 provides a robustness analysis. Finally, section 7 concludes.

2.2 Data

Our empirical strategy relies on a well defined measure of risk taking and on comparability of the treatment and the control group. For the purpose of building a risk measure (i.e. an elasticity of loan origination to Loan-to-Income) our main source of data are the Home Mortgage Disclosure Act Loan Application Registries which contains the data on mortgage applications. To ensure comparability of the treatment and control groups we use the FDIC Call reports which contain the bank balance data.

The Home Mortgage Disclosure Act (HMDA) obliges banks above a set threshold of assets to report on mortgage applications. The yearly Loan Application Registries of banks which meet the criteria to report, contain all mortgage loan applications, the properties of the applicant and potential co-applicant (ethnicity, race, gender, income), the loan properties (amount, type, purpose, rate spread for some, occupancy), the properties of the house (type, census tract, etc.), the properties of the census tract (income relative to the relevant Metropolitan Statistical Area, minority population, number of housing units, etc.), and the action taken (origination, denial and its reason, purchase, etc.)[1].

For the purpose of estimating the elasticity of mortgage origination to the loan to income ratio of the applicant we define origination as an application which has been accepted and then either originated or refused by the applicant, a purchase of a loan, or a pre-approved request. We define a non-origination as an application denied by the bank or a denied pre-request. We ignore all applications withdrawn by the applicants or applications closed for incompleteness. We also restrict the analysis to mortgage loans with the purpose of home purchase or refinancing, and we exclude home improvement.

The loan to income ratio is computed as the total loan amount in the application over the total gross annual income an institution relied upon in making the credit decision[2].

[1] The data for 2013-2015 is available on `https://www.ffiec.gov/hmda/hmdaflat.htm`. The data for 1981 to 2012 is available at national archives.

[2] Gross annual income is not registered in HMDA in the following cases: (i.) multifamily dwellings, (ii.) income was not registered in the loan purchase documentation, (iii.) loans to bank employees, (iv.) loans to non natural persons. These cases are excluded from the estimation as described in the methodology section

The second important source of data is the balance sheet data. This data is used to assure the comparability of the treatment and the control group and for the purpose of controlling for bank characteristics in the final regression. The source of balance sheet data are the FDIC Call Reports. They are available at quarterly frequency for the population of FDIC insured banks. The subject of our analysis are the state chartered banks, all of which are also FDIC insured.

Table 2.1: Balance sheet statistics for all FDIC insured banks, the state charetered banks and Massachusetts state chartered banks

	All Banks			State. Chrt. Banks			MA St. Chrt. Banks		
	01-06	07-12	12-16	01-06	07-12	12-16	01-06	07-12	12-16
Size									
assets	1029	1642	2320	697	748	923	568	756	950
# empl.	226	263	314	143	122	136	121	138	156
Performance and condition ratios									
cap.rat.	.12	.11	.11	.11	.11	.12	.1	.1	.11
ltd	6.32	13.65	.99	.82	.86	.84	.78	.87	.89
roa	1.02	.49	.93	.71	.2	.53	.72	.37	.51
net.int.mar.	4.08	3.83	3.68	3.46	3.2	3.31	3.34	3.1	3.17
Lending									
lta	.62	.64	.6	.63	.66	.67	.6	.65	.7
residential	.35	.33	.35	.71	.66	.66	.73	.67	.63
Liabilities									
dep.to ass	.81	.81	.83	.78	.78	.8	.79	.77	.79
retail	.82	.84	.92	.87	.86	.93	.87	.85	.92
insured	.79	.81	.83	.86	.87	.88	.81	.83	.82

This table contains the means of some descriptive variables for all the FDIC insured banks, the state chartered banks and the Massachusetts state chartered banks by years. *Assets* are total assets in millions of dollars, *# Empl* denotes the number of employees, *Cap.rat.* denotes the leverage (capital to assets) ratio, *LTD* denotes the loan to deposit ratio, *roa* denotes the return on assets, *net.int.mar* denotes the net interest margin, *lta* denotes the loan to assets ratio. *residential* denotes the share of residential loans in total loans, *dep. to ass.* denotes the deposits to assets ratio, *retail* denotes the share of retail deposits in total deposits, and *insured* denotes the estimated share of deposits insured by the FDIC.

Table 2.1 provides some descriptive statistics on both the population of FDIC insured banks and the subpopulation of state chartered FDIC insured banks. State chartered banks are on average smaller than the the average of all FDIC insured banks, both in terms of assets and the number of employees. They are also on average less profitable and operate with a lower net interest margin. They are similar in terms of the structure of their liabilities, but they do differ substantially regarding their loan portfolio where the state chartered banks focus much more on residential mortgage lending, which is evident from their high share of residential loans in total loans.

As will be explained in more detail below, we build a treatment group to resemble the Massachusetts state chartered banks. The share of non-insured deposits in the liabilities of Massachusetts state chartered banks and in the treatment group, around 20% of deposits, provides enough leverage to affect bank behaviour.

2.3 Methodology

The causal effect of deposit insurance on risk taking is estimated using difference-in-differences, where we regard treatment as an increase in deposit insurance coverage. The validity of the methodology relies crucially upon three steps. First, to assure random assignment of treatment, we make use of a natural experimental setting, where the limit of the deposit insurance coverage was raised for some banks but not for all. Secondly, to assure that banks in the treatment and control group are comparable, we resort to nearest neighbour matching based on covariates which might affect risk taking. Finally, to get a proper estimate on of the effect on risk taking behaviour, we construct a risk taking measure, based on new loan issuances, which differs from conventional risk measures which are based on the existing portfolio of loans.

2.3.1 Identification - natural experiment

Policy actions regarding deposit insurance coverage, which are implemented as a result of the build-up of risk, can render a biased estimation of their effect on risk taking due to reverse causality. To estimate the effect, a control group, unaffected by the policy due to reasons exogenous to their behaviour, is needed. In this paper we exploit an experimental setup from the deposit insurance system in the US.

In the US, deposit insurance is carried out by the FDIC (Federal Deposit Insurance Corporation), which was set up in 1933. In addition to this Federal Deposit Insurance, since 1934 state-chartered savings banks in the State of Massachusetts are covered by a private deposit insurance company, the DIF [3] (Depositors Insurance Fund), which offers *unlimited* insurance on deposits of member banks. Membership to this insurance scheme is mandatory for all state chartered banks in Massachusetts.

On October 3rd, 2008, the FDIC increased the statutory coverage from $100,000 per depositor to $250,000. This was intended to be a temporary measure, but the decision became permanent on July 21st, 2010. Since DIF members[4] in Massachusetts always had unlimited coverage, they are unaffected by this change.

The fact that the state-chartered banks from the State of Massachusetts are unaffected by the increase in coverage enables us to use these banks as a control group in our estimation of the treatment effect. To assure similar regulatory framework, we focus our analysis on state-chartered banks only, and thus define the treatment group as state-chartered banks in other states where the policy would have an effect.

Using this framework, we use a difference-in-differences approach to examine how bank risk taking changes for the treated banks as a response to higher deposit insurance coverage, in comparison to that of the banks in the control group.

2.3.2 Identification - matching

With the control group defined by the fact that state-chartered banks in Massachusetts are unaffected by an increase in deposit insurance coverage, we are assured that the immunity to deposit insurance coverage increases is exogenous to the behaviour of these banks and thus equivalent to random assignment. Equal regulatory framework is ensured by focusing on other state-chartered banks to use as a treatment group. However, to achieve comparability of the treatment and the control group, the treatment group needs to be narrowed further to banks which share the characteristics of the control group.

Matching on observables before treatment allows us to obtain a list of banks which share the characteristics of those in the control group. For each bank in the control group, we therefore find three matching banks in the pool of treated [5]. The matching methodology is nearest neighbor

[3] Not to be confused with the Deposit Insurance Fund, (whose initials are also DIF), which is one of the funds through which the FDIC carries out insurance.

[4] We track DIF membership yearly since 2000

[5] The choice of number of matches is based on the fact that there are 50 banks in the control group. We aim to have more than one match for each in order to have a larger number of observations. Results are robust to other numbers of matches.

matching. We match on the pre-treatment averages of balance sheet size, leverage ratio, capital to asset ratio and deposit to loan ratio. For balance sheet size we match exactly. These variables have been shown in the literature to explain risk taking by banks. It is therefore crucial that they are balanced across the treatment and the control group.

Table 2.2 shows the mean of some key variables for the treatment and the control group before the treatment date, together with the p-values for the two sample t-test for the means[6].

Table 2.2: Bank characteristics before treatment

Variable	Control	Treated	p-value
	Financial Variables		
assets	621,685.1	655,719.34	.11
car	.1049	.1065	.12
leverage	8.86	8.74	.14
loantodep	.7925	.8459	.00
net interest margin	3.22	3.34	.00
	Loan Composition		
residential	.7081	.6703	.00
commercial	.0486	.0499	.44
individual	.0250	.0503	.00
	Deposit Composition		
total deposits	485,449.57	488,909.67	.82
transaction	.1502	.1466	.21
non transaction	.8497	.8533	.21
retail	.8524	.8736	.00
deposits below 100k$	.7958	.8606	.00
deposits by banks	.0017	.0027	.00

This table contains the means of some descriptive variables for the control and the treatment group before the treatment date. *assets* are total assets in thousands of dollars. *car* denotes the capital-to-asset ratio. Leverage is defined as total debt over total equity. *loantodep* is total loans over total deposits. Loan composition variables consist of the ratios of residential loans, commercial loans and individual loans to total loans. Deposit composition variables consist of total deposits in thousands of dollars, and the ratios of transaction accounts (demand deposits, etc.) and non transaction accounts (savings deposits, time deposits, etc), deposits below the pre-treatment coverage limit of $100k, and deposits by other banks to total deposits.

Table 2.2 provides evidence that both groups are similar in observables in the pre-treatment period. Given the list of variables that the matching is done upon, it is unsurprising that they have similar balance sheet size, capital to asset ratios and leverage ratios. In addition to that, their balance sheet composition is very similar. A large share of the loan activity of both groups is allocated to residential loans (i.e. mortgage loans), and most of their deposit base comes from retail deposits.

The total amount of deposits held by both groups is not statistically different between the two groups. Likewise, the difference in the share of deposits which are held in transaction accounts (with higher liquidity) or non transaction accounts (with lower liquidity) between the treatment and the control group is statistically insignificant. This indicates that both groups face a similar liquidity structure in their funding sources, which is important towards their ability for maturity transformation. Furthermore, it is worth noting that from December 2010, to December 2012, transaction accounts that pay no interest were entitled to unlimited deposit insurance coverage by the FDIC, as commanded by Congress. This could pose an issue for our experiment if the share of

[6]The null hypothesis is that the means are equal, hence a p-value higher than 0.05 indicates that the means for both groups are not statistically different at the 95% confidence level

such accounts was a large enough part of our banks' balance sheets. This is not the case as shown in Table 2.2.

Other variables, such as the loan-to-deposit ratio, the share of residential loans to total loans, net interest margin and share of retail deposits to total deposits are statistically different, but qualitatively similar. One variable that is especially important is the share of total deposits below the pre-treatment limit of $100,000. We could expect that depositors with larger balances prefer to deposit them in MA banks, due to higher coverage. This could pose another issue for our experiment if, after the treatment, these depositors decided to move their funds to another bank. In that case the treatment would indirectly affect the control group through deposit supply. We address this issue in the Robustness section and show that it is not the case.

Furthermore, due to an overlap of the timing of the policy implementation and the start of the financial crisis, any significant differences in the characteristics between the two groups, would potentially plague our results due to a different reaction of the groups to the crisis. This exercise, however, confirms that the balance sheet structure of both groups is similar enough to ensure that when facing a shock, such as the financial crisis, they would be affected in a similar manner.

Finally, we note that some significant banking regulation changes, such as the Dodd-Frank Act, took place during the period of our study. However, insofar as these changes were done at the Federal level, they affect both the treatment and the control group equally and this is picked up by the difference in differences estimator.

2.3.3 A measure of risk taking

To analyse bank risk taking it is important to rely on a risk measure which is not tainted by past choices and current shocks to their portfolio. If instead a measure of risk based on the current portfolio is used, any asymmetry in the severity of the financial crisis could affect different portfolios differently and thus meddle with the effect which stems from new choices. As discussed earlier, a measure of risk taking based on new issuances of loans does not have this problem. Variation in such a measure results from the choices of banks with regards to the expected performance of loans and borrowers at the point of issuance and not on the ex post performance of loans issued in the past. To this end, we construct a measure of risk taking on the issuances of new mortgage loans based on the Home Mortgage Disclosure Act (HMDA) dataset as it provides information on the population of mortgage applications to banks and other mortgage lenders, including detailed information on the borrower and loan characteristics. Given the high share of residential loans in the portfolios of state chartered banks, which are at the focus of our analysis, we take the risk of new mortgage lending as representative of risk taking on the entire portfolio.

To construct a measure of banks inclination to take risk, we estimate a propensity to originate the loan given the loan risk characteristics. DellAriccia et al. (2012) and Ignatowski and Korte (2014), among others, have shown that loan-to-income ratios are a good proxy for riskiness of loans. Following this idea, we measure the risk connected to the issuance of a loan and to the borrower using the loan-to-income ratio (LtI) measure computed from the HMDA data set for every loan application. We construct a new measure of risk taking through the following model[7].[8]

$$Origin_{t,i,j} = \gamma_t^0 + \gamma_{t,i}^1 LtI_{t,i,j} + \epsilon_{t,i,j} \tag{2.1}$$

where $Origin_{t,i,j}$ denotes a binary loan origination variable which takes the value $Origin_{t,i,j} = 1$ if application in period t to a bank i by a borrower j is accepted and loan is originated, and takes the value $Origin_{t,i,j} = 0$ if the application is rejected and the loan is not originated. Time effects, γ_t^0, capture the effect of the macroeconomic situation in period t for all banks, such as market liquidity, regulation, and monetary policy, among other things. Finally, for every bank i in every period t we also obtain an estimate of the sensitivity of origination to the risk associated with a loan $\gamma_{t,i}^1$ based on loan-to-income ratio of all applicants j from 1 to J, which serves as a measure of risk-taking by banks.

Figure 2.1: Distribution of the risk measure for all the FDIC insured banks and the Massachusetts state chartered banks.

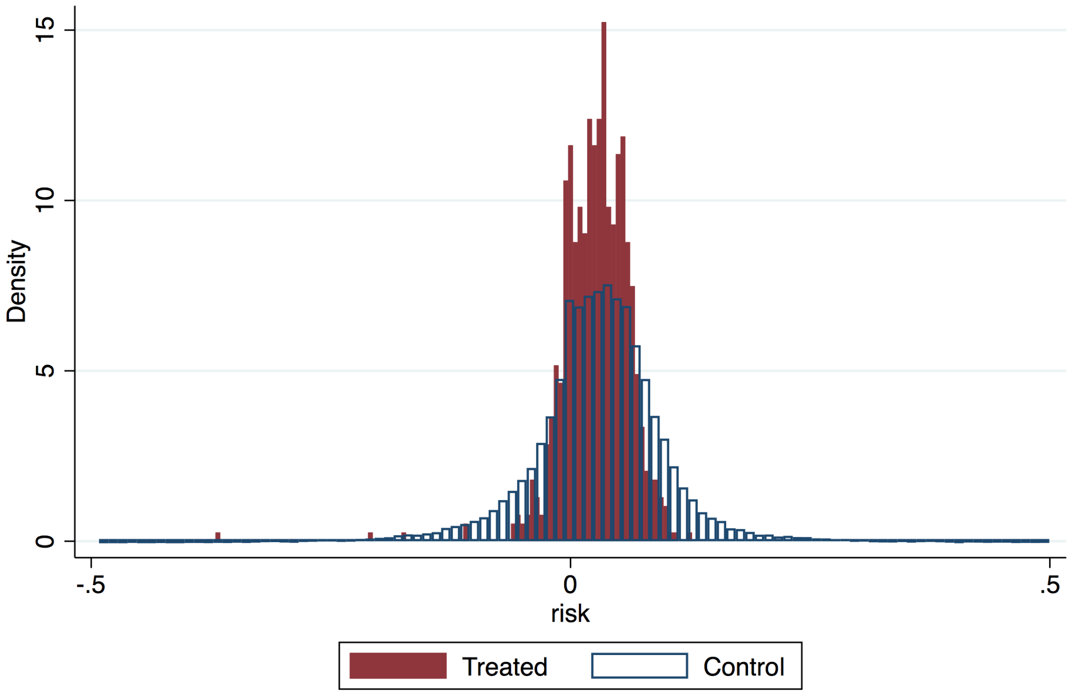


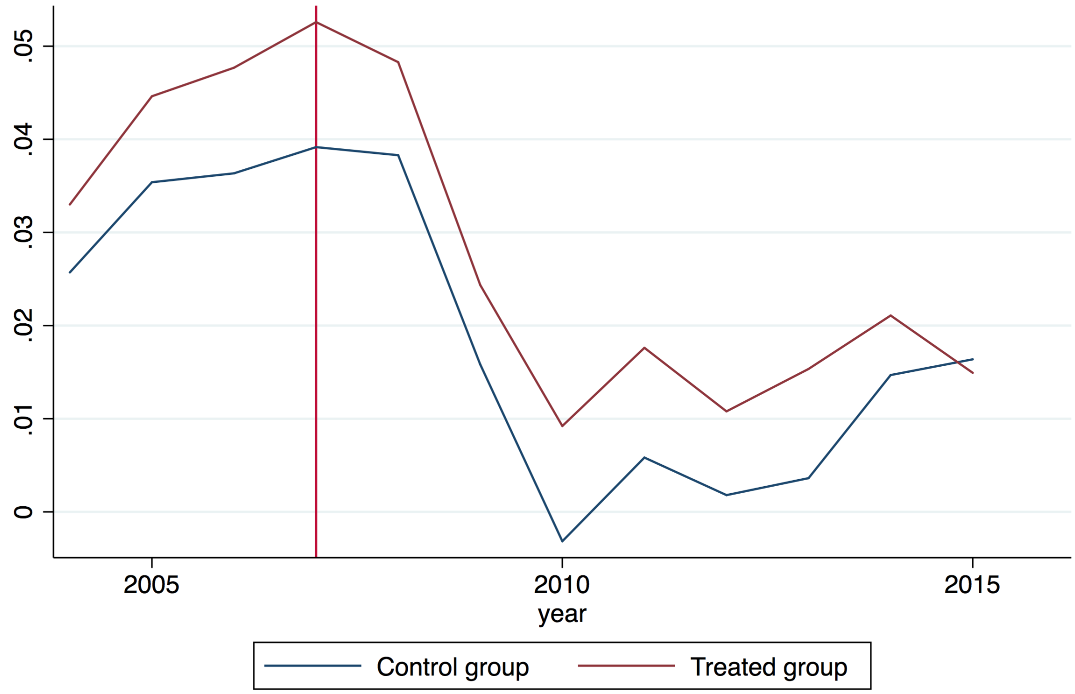
Figure 2.1 plots the risk sensitivity parameters for the banks included in the analysis over the years.

While we believe that the risk taking measure, as constructed above, has deficiencies, we also believe that our results are robust to those deficiencies. Firstly, as is documented in Ferrari et al. (2018), out measure predicts well the county level default rate. Furthermore, the results using

[7]In order for γ_t^0 to capture the macroeconomic conditions affecting the origination choices, we estimate the model for all banks reporting to the HMDA data set but of course only use the $\gamma_{t,i}^1$ for banks included in the final regressions. This implies including all the loan applications in the HMDA reporting in the estimations. The number varies between 17 million applications and 40 million application which constrains us to estimating the model as a linear probability model

[8]This is joint work with Ferrari et al. (2018).

Figure 2.2: Means of the risk measure for all the FDIC insured banks and the Massachusetts state chartered banks.



implicit deposit interest rates give credence to the conclusions we are inferring from the risk taking measure.

2.3.4 Difference-in-differences

In estimating the causal effect of the increase in deposit insurance coverage on risk taking we use the difference-in-differences approach. This implies estimating the following model:

$$\gamma^1_{t,i} = \beta_0 + \beta_1 D_T + \beta_2 D_{after} + \beta_3 D_T D_{after} + \epsilon_{it} \tag{2.2}$$

where D_T is a dummy variable that takes value 1 for the treated and 0 otherwise, and D_{after} is a dummy variable that takes value 1 for the treatment period and 0 otherwise. Hence β_3, the coefficient for the interaction term between the treated dummy and the treatment period dummy, is the average treatment effect on the treated, i.e. the causal effect of the policy change on risk taking behaviour.

The key assumption in the difference-in-differences estimator is that, in the absence of treatment, both groups would have followed similar time trends. Figure 2.2 plots the means of the risk taking measure for the control and the treatment group. The figure indicates that the two groups behaved similarly in the pre-treatment period regarding their risk taking. This implies that we are evaluating the effect of the policy on a comparable set of banks and that the results of the estimation of equation 2.2 will provide the estimate of the causal effect (or as the plot suggest the absence of one).

In our exercise to determine the cause of no causal effect, we will apply the same methodology to estimating the effect of an increase of deposit insurance coverage on the price of deposit funding for banks. The validity of the common trend assumption in the pricing variable is confirmed in section 2.5 which also provides the results.

2.4 Results - risk taking

We begin by providing the results of the estimation of equation 2.2. Columns (1) to (4) provide the results using several specifications. Columns (1) and (2) provide the results for the OLS specification and bank fixed effects specification. Since our focus is on the state-chartered banks, to control for possible regulatory and other institutional or structural differences across states, we include also state-fixed effects (see column (3)). Furthermore, to control for a different severity of the crisis, we add a specification with state-year fixed effects (see column (4)). The dependent variable across all specifications is our risk measure, the sensitivity of loan origination to loan-to-income ratio, constructed as explained in the preceding section.

Table 2.3: Results for risk measure based on loan applications

VARIABLES	(1) $\gamma^1_{t,i}$	(2) $\gamma^1_{t,i}$	(3) $\gamma^1_{t,i}$	(4) $\gamma^1_{t,i}$
D_T	0.0104***	0.0110	0.0165***	-0.00339
	(0.00304)	(0.00929)	(0.00376)	(0.0105)
D_{after}	-0.0223***	-0.0215***	-0.0223***	-0.0226***
	(0.00334)	(0.00278)	(0.00336)	(0.00721)
$D_T D_{after}$	-0.00172	-0.00278	-0.00162	-0.00426
	(0.00441)	(0.00343)	(0.00428)	(0.0117)
Constant	0.0342***	0.0112	0.0342***	0.0392***
	(0.00205)	(0.00765)	(0.00207)	(0.00493)
Observations	1,451	1,451	1,451	1,451
R-squared	0.073	0.558	0.175	0.290
Bank FE	NO	YES	NO	NO
State FE	NO	NO	YES	NO
State x Year FE	NO	NO	NO	YES

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

In all specifications, the coefficient of interest, $D_T D_{after}$, corresponding to β_3 in equation 2.2, is insignificant. This suggests that an increase in deposit insurance coverage limit does not cause banks to take on more risk. The estimation is consistent with the graphical evidence from Figure 2.2, where in the post-treatment periods, the means of the two groups did not diverge.

The results of our empirical analysis do not confirm the results from the previous studies. Using an experimental setup and a measure of risk-taking that is based on new loans instead of balance sheet data, we find no significant effect of an increase of deposit insurance coverage limit on risk taking by banks. This result is important for policy making insofar as, when faced with the trade off between the risk of bank runs or the higher systemic risk induced by moral hazard, the regulator should take into consideration that the moral hazard channel may not be an issue.

It is worth pointing out that the treatment in our experiment is an increase in deposit insurance coverage, hence these results apply to the intensive margin. While it may be true that implementation of deposit insurance may increase the problem of moral hazard, it appears that moral hazard is not a concern when we consider increases in coverage. It should be stressed, however, that although this policy is increasing the insurance coverage and not establishing deposit insurance from zero, it does affect depositors with deposits in excess of $ 100,000, who tend to be more informed and knowledgeable in financial diversification. This limits the argument that the policy along the intensive margin would not provide enough of an incentive to apply market discipline.

2.4.1 The Crisis

As pointed out by Anginer et al. (2014b), deposit insurance can induce moral hazard during normal times, but its stabilizing effect may outweigh the moral hazard effect during times of financial turmoil. This would suggest that in periods of stress, stable funding allows banks to invest in safer assets. Furthermore, periods of financial stress correspond to periods of intense supervisory monitoring, which would limit the scope of banks to take on excessive risk.

In order to test whether the treatment effect is different during the crisis, we add a dummy variable for the crisis period and an interaction term between the crisis period and the treated group to regression 2.2[9].

$$\begin{aligned}\gamma^1_{t,i} &= \beta_0 + \beta_1 D_T + \beta_2 D_{after} + \beta_3 D_T D_{after} \\ &+ \beta_4 D_{crisis} + \beta_5 D_T D_{crisis} + \epsilon_{it}\end{aligned} \tag{2.3}$$

Here, β_4 reflects the effect of the crisis on bank risk, and β_5 is the coefficient for the treatment effect of deposit insurance during the crisis.

Table 2.4 shows the results of the analysis including the crisis period interaction as described above. Column (1) corresponds to the standard difference-in-differences regression reported in the first column of Table 2.3, while Column (2) includes the crisis specification.

The coefficients of interest, $D_T D_{crisis}$, corresponding to β_5 in equation 2.3 is not statistically significant. This implies that the effect is insignificant in both crisis and non-crisis periods. This result is also important as it indicates that overall result of no effect of deposit insurance coverage limit is not due to the fact that in crisis and non-crisis subperiods the effects have an opposite sign as per Anginer et al. (2014b).

It is worth pointing out that the timing of the crisis and the that of the policy announcements poses some difficulties. Recall that it was in October 2008 when the FDIC announced a temporary increase in the deposit insurance coverage, which was supposed to last until December 2010. In July 2010, the FDIC announced that this measure would become permanent. Arguably, October 2008 was the peak of the financial crisis, with Lehman Brothers filing for bankruptcy in September of that year. Furthermore, by July 2010 the crisis in the US was coming to an end. The coincidence in time of the policy announcements and the crisis period makes it hard to disentangle whether the different treatment effect during these years is due to the financial crisis, or to its temporary or permanent nature. We address this issue later on in the robustness section.

2.5 Results - pricing and market discipline

We have so far established that an increase in deposit insurance coverage does not cause an increase in risk taking, disproving one of the more commonly cited potential pitfalls in providing

[9] In this specification, an interaction term $D_{after} D_{crisis}$ should be added. However, the treatment period and the crisis period overlap (i.e. the crisis period is fully contained in the after treatment period), hence this interaction is collinear and therefore redundant.

Table 2.4: Results for crisis period specification

VARIABLES	(1) $\gamma^1_{t,i}$	(2) $\gamma^1_{t,i}$
D_T	0.0104***	0.0104***
	(0.00304)	(0.00304)
D_{after}	-0.0223***	-0.0256***
	(0.00334)	(0.00350)
$D_T D_{after}$	-0.00172	-0.00309
	(0.00441)	(0.00467)
D_{crisis}		0.00813
		(0.00574)
$D_T D_{crisis}$		0.00343
		(0.00687)
Constant	0.0342***	0.0342***
	(0.00205)	(0.00205)
Observations	1,451	1,451
R-squared	0.073	0.082

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

deposit insurance or increasing its coverage. As theory predicts, the moral hazard effect of an increase in the coverage of deposit insurance is that deposit supply increases and depositors reduce their monitoring of bank behaviour. Increased deposit supply and laxer market discipline should, given a level of demand for deposit funding, decrease the interest rate on deposits.

To test whether an increase in deposit insurance coverage affects the deposit supply and market discipline we use the difference-in-differences estimation. Theoretical prediction states that an increase in deposit insurance coverage decreases deposit rates as a result of the deposit supply rise. We therefore run the following regression:

$$i_{t,i} = \beta_0 + \beta_1 D_T + \beta_2 D_{after} + \beta_3 D_T D_{after} + \epsilon_{it} \tag{2.4}$$

where $i_{t,i}$ refers to the deposit rate of bank i in period t. As before, the coefficient of interest is β_3, which provides the estimate of the treatment effect of an increase in deposit insurance coverage on the deposit interest rates. Since we do not have data on deposit rates for new deposit transactions, following Schmukler (2001), we compute the implicit interest rate, i.e. the ratio of total interest expense on domestic deposits over total domestic deposits for each bank each year. In using implicit deposit rates, we may be overlooking deposit composition effects. It cannot be excluded that deposits with different maturities are subject to different rates. Averaging interest expenses across deposit classes may therefore be an issue.

Figure 2.3 plots the means of the implicit deposit interest rates over the horizon for the control group and the treatment group as defined in the previous section. The figure provides several insights. Firstly, the two series clearly exhibit a common trend in the pre-treatment periods, suggesting that the series are suitable to perform the estimation on. Secondly, the lack of divergence in the post-treatment periods already hints at the conclusions which are confirmed by our estimation results in Table 2.5.

Figure 2.3: Means of the implicit deposit rates for all the FDIC insured banks and the Massachusetts state chartered banks.

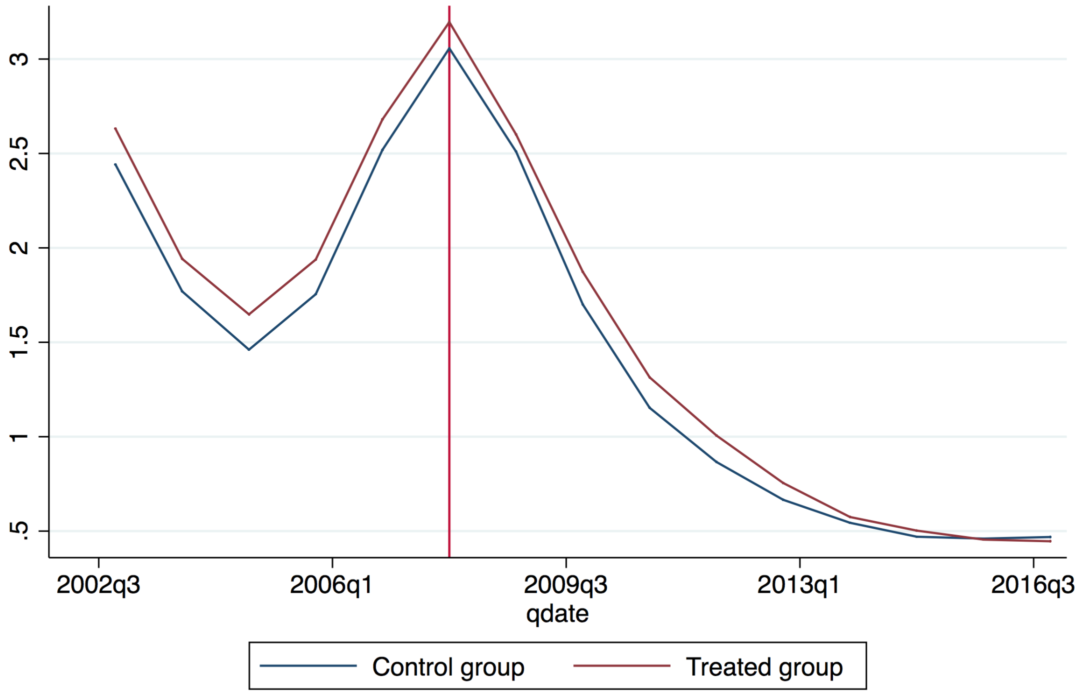


Table 2.5: Effect of deposit insurance on deposit rates

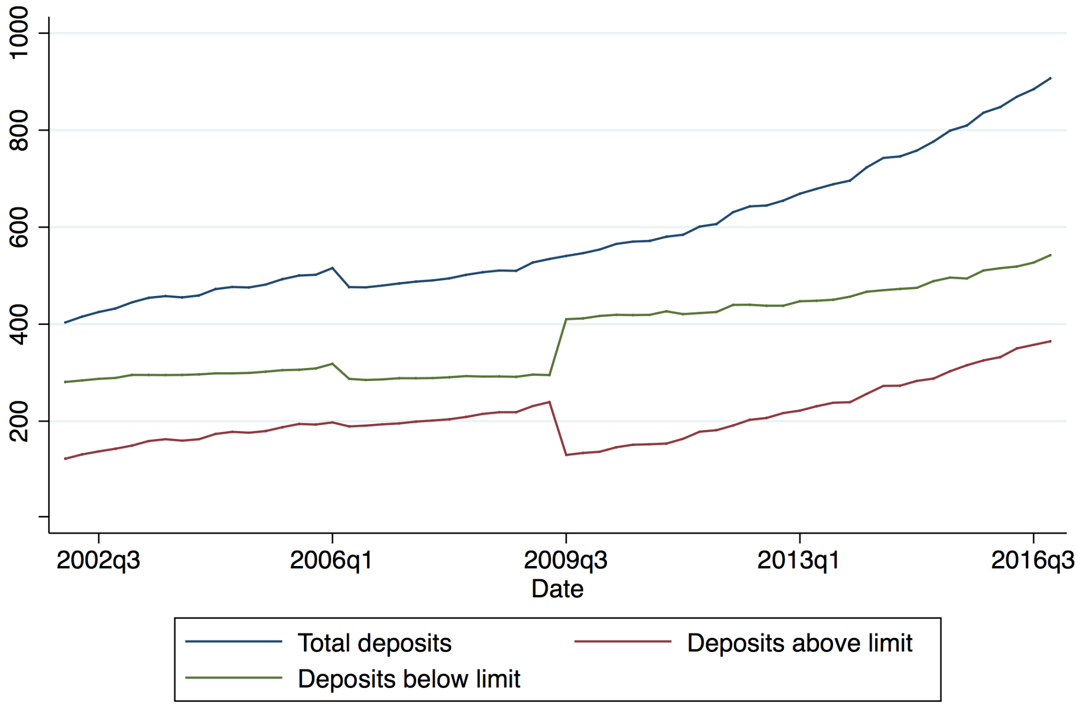
VARIABLES	(1) DEPOSIT RATE
D_T	0.178***
	(0.0488)
D_{after}	-1.131***
	(0.0521)
$D_T D_{after}$	-0.104
	(0.0670)
Constant	2.167***
	(0.0375)
Observations	2,028
R-squared	0.393

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

The coefficient of interest, $D_T D_{after}$, corresponding to β_3 in equation 2.4, is statistically insignificant, indicating that the increase in deposit insurance coverage does not increase deposit supply which would result in lower interest rate.

This result provides an explanation for the result of no response of bank risk taking following an increase in deposit insurance coverage, presented in the previous section. After in increase in deposit

Figure 2.4: Deposits of the Massachusetts state chartered banks.



insurance coverage, the depositors do not increase their supply of funding to the bank which does not result in laxer market discipline. Our results are in line with those of Schmukler (2001).

2.6 Robustness

2.6.1 Indirect treatment effect on the non-treated through the deposit supply

Our results rely on the assumptions of matching and the difference-in-differences approaches. We have established a treatment and a control group of banks that are similar in observables. However, to assure their validity, we need to make sure that the control group is unaffected by the treatment. The control group in our experiment is comprised of banks whose deposit insurance is unlimited, which implies that they are unaffected by the country-wide increase in the national coverage limit from \$100,000 to \$250,000. Although we see no way for the control group to be affected directly by the treatment, there remains a plausible indirect effect through deposit supply. In this case depositors in Massachusetts state chartered banks, whose choice of bank was based on the fact that their funds are insured, could withdraw their funds in excess of \$100,000 and opt for a bank which now also offers insured deposits but is more convenient along some other dimension.

Figure 2.4 shows the average total balance of deposits for each bank, and how they are split between those which lie above the insurance limit and those below. The two kinks in the amount of deposits above and below the limit correspond to the change in policy. Note that with the increase in coverage, deposits between \$100,000 and \$250,000 were above the limit before the change, and are below the limit after, so the amount of deposits below the limit increases merely by accounting. The same logic can be applied to deposits above the limit. However, the total amount of deposits does not deviate from its trend. It can thus be concluded that the banks in the control group did not experience any deposit flight due to the policy change.

2.6.2 Results for different treatment dates

As stated before, the increase in deposit insurance that happened in 2008 was intended as a temporary measure. However, it was made permanent in July 2010. In order to make sure that the absence of a treatment effect is not due to the temporary nature of this measure, we estimate the same model with 2010 as a treatment date.

Furthermore, in order to ensure that the lack of effect is not due to anticipation effects, we use 2007 as a treatment year, to account for the case that some information regarding an increase in deposit insurance coverage circulated prior to the implementation.

Table 2.6: Different treatment dates

	(1)	(2)
VARIABLES	Treatment in 2007	Treatment in 2010
D_T	0.00936***	0.00989***
	(0.00334)	(0.00270)
D_{after}	-0.0175***	-0.0254***
	(0.00332)	(0.00375)
$D_T D_{after}$	-4.93e-05	-0.00160
	(0.00448)	(0.00466)
Constant	0.0325***	0.0318***
	(0.00227)	(0.00187)
Observations	1,451	1,451
R-squared	0.040	0.099

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 2.6 shows that the treatment effect is still insignificant for these treatment dates. Again, we find no evidence of a moral hazard problem that would result in higher risk taking by banks after an increase in deposit insurance coverage.

2.7 Conclusion

In this paper we estimate the effect of deposit insurance on risk taking by banks. It is a well established theoretical result that deposit insurance disincentivises depositors to monitor bank behaviour since it assures depositors access to their liquid assets independent of bank risk. This incentivises banks to take on more risk.

To test these predictions we use a natural experiment setting from the US deposit insurance system, where state chartered banks in Massachusetts were immune to an increase in deposit insurance coverage limit by the FDIC due to unlimited deposit insurance provided in local regulation. Furthermore, to avoid any biases arising from using risk measures based on the existing portfolios, we use mortgage lending data to construct a measure of risk taking based on new issuances.

Our results indicate that an increase in deposit insurance coverage has no effect on risk taking by banks. Furthermore, in exploring the reasons of no effect on deposit insurance, we find that an increase in deposit insurance coverage also has no effect on deposit interest rates. This indicates that deposit insurance does not relax market disciplining by depositors through an increase in deposit supply. These results imply that the usually quoted negative implications of deposit insurance are not an issue in practice and should therefore be disregarded when considering different deposit insurance schemes.

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Chapter 3

Local Effects of Monetary Policy

The Role of Local Banks in the US in the Post Crisis period

3.1 Introduction

The data suggests that there is a substantial amount of disparity in the cyclicality of employment across different geographical areas in the United States. While fiscal policies differ across areas, defined as granular as municipalities, there is only one monetary policy which targets aggregate economic outcomes. However, a single monetary policy can have different effects on the economic outcomes, depending on the characteristics of local economies. An important characteristic is also the type of banks which operate in a particular area.

In this paper we estimate the effect of the characteristics of banks operating in a particular area on the impact of the monetary policy on the local economic outcomes. We rely on the data on employment statistics at the county level to measure local economic outcomes. We then aggregate the characteristics of banks to a county level using their share of deposits in total amount of deposits in that county.

Through this approach we contribute to to two distinct strands of literature. On the one hand, it adds to the literature with a focus on the transmission channel of monetary policy and the bank characteristics that affect it. On the other hand it contributes to understanding differential local responses to monetary policy.

It is now a well established empirical and theoretical finding that monetary policy affects real output and that an important channel of the transmission runs through credit. Bernanke and Gertler (1995) propose a bank lending channel where a tightening of monetary policy decreases the supply of loanable funds and reduces the provision of funding to the economy.

Empirical studies have provided mixed results on the validity of these predictions. Kashyap and Stein (1995, 2000) argue that banks have to replace the lost deposits and raise alternative funds with large certificates of deposits or in the interbank lending market. They show that larger banks and banks with higher market power are less constrained in attaining alternative sources of funding and therefore limit their lending less than smaller banks which are more contstrained. These predictions have been confirmed by several studies. For example, Ashcraft (2006) finds that the response of lending to monetary policy is stronger in state banking markets where financially constrained banks have a higher market share, but that there is little difference in the response of state output, implying that the aggregate elasticity of output to bank lending is very small, if not zero. Carlino and Defina (1998) estimate a structural VAR model at the regional level and find that a core of regions (New England, Mideast, Plains, Southeast, and the Far West) respond to monetary policy changes in ways that closely approximate the U.S. average response but that Southwest and Rocky Mountains are found to be much less sensitive. Contrary to the above conclusions they also find that regions become less sensitive to a monetary policy shock as the percent of small banks increases. They argue that this is due to the fact that size is a not an optimal measure of access to finance as opposed to bank capital. More recently, Neville et al. (2012) estimate the city-level responses to monetary policy shocks in a Bayesian VAR and conclude that bank lending channel only marginally explains the differential responses to monetary policy but that instead demographic characteristics and the size of local government play a crucial role. Adams and Amel (2011) on the other hand check if the concentration of local banking markets affects the response of lending to monetary policy. Their results indicate that more concentrated markets have lower business loan originations and experience smaller changes in these originations in response to changes in the federal funds rate supporting the idea that market concentration dampens quantity reactions to input price changes.

Heterogeneity of the results in the literature and a strong focus on effect of size and capital structure of the banks on the transmission of monetary policy provides scope for our analysis. There are several results of our empirical exercise which contribute meaningfully to the above-mentioned literature. First, as expected, a tightening of the monetary policy decreases employment, the total annual payroll and the average annual payroll also at the county level. Second, we show that local responses differ depending on the characteristics of local banks. Contrary to the predictions of Kashyap and Stein (1995, 2000) we cannot confirm that the effect is influenced by the average size of the bank in an area. However, as the capital structure of local banks improves the effect is dampened. This result goes in line with a prediction that well capitalised banks find it easier to attain alternative sources of funding after a monetary tightening. Finally, and most importantly, we find that as risk associated with local banks increases, this intensifies the effect of the monetary policy. We rationalise this finding with the fact that not only well capitalised, but also less risky banks have access to alternative sources of funding, which limits their decrease in loanable funds. This novel finding has important policy implications. It suggests that any countercyclical policies which limit credit risk exposures or induce a build-up of capital in upswings also decrease the sensitivity of employment and payrolls to a monetary tightening.

This paper proceeds as follows. Section 2 describes the data and the aggregating process of bank characteristics to a county level. Section 3 describes the empirical methodology. Section 4 presents the results and is followed by Section 5 which discusses the results, and reflects on the theoretical predictions which justify the main findings and the potential policy implications. Finally, section 6 concludes.

3.2 Data

3.2.1 Local economic outcome data

To measure the effect of bank characteristics on the transmission of the monetary policy at the county level we first need to define local economic outcomes. Hoai-Luu (forthcoming) argues that while mortgage lending in United States tend to be less localised, banks focus their corporate lending to firms in the proximity of their offices. For this reason we focus our analysis on employment and payroll data based on the county of residence of the firm from the County business patterns (CBP) data. CBP is an annual series that provides subnational economic data and includes the number of establishments, employment during the week of March 12, first quarter payroll, and annual payroll. We collect data on total employment and total annual payroll.

Figure 3.1: Growth rate of annual employment, payroll, average payroll and unemployment rate

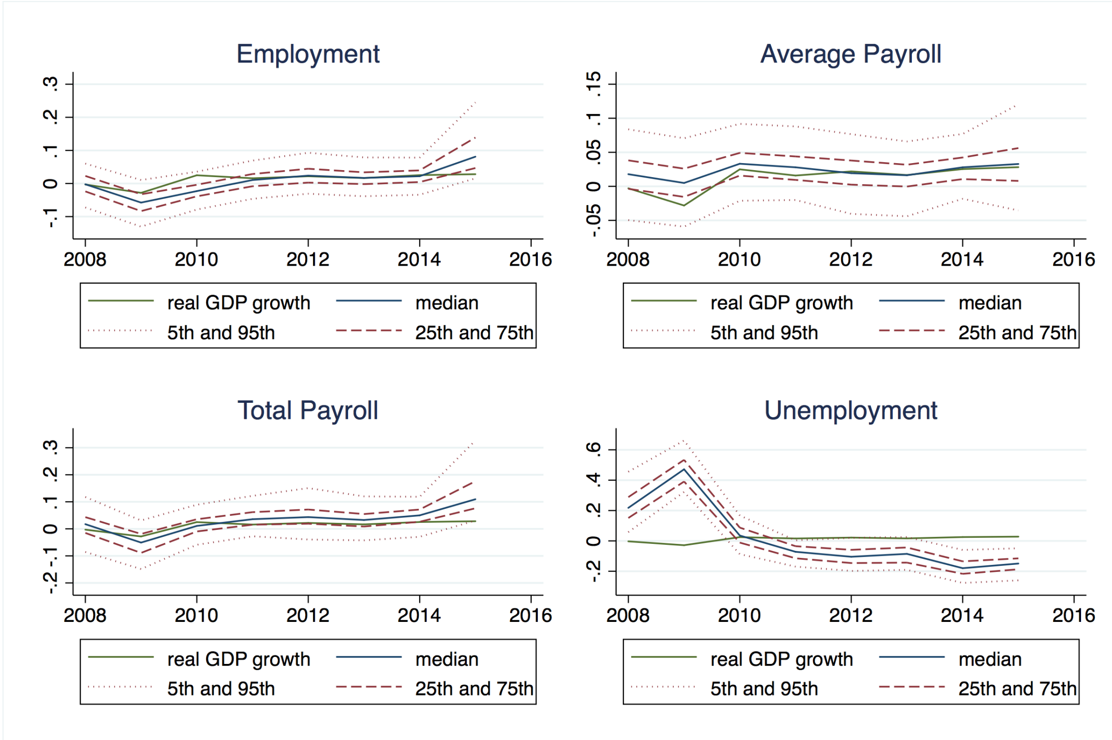


Figure 3.1 plots the median, and the relevant percentiles of the growth rate of employment, annual payroll, average annual payroll and unemployment across time. The employment, total annual payroll of the median county clearly follows the aggregate real GDP. We can also observe a substantial degree of heterogeneity in the growth rates across counties. For instance, whereas the median county in 2009 experienced a 6% decrease in employment, the 75th percentile county experienced an only 3 % reduction and the 95th percentile county experienced a 6% increase in employment. Even more starkly, total annual payroll growth rate varied between -3% and 13%. This variation gives us enough scope to study potentially different responses of local employment and payrolls to monetary policy changes.

3.2.2 Bank data

We use two data sources for the characteristics of banks at the county level. First, the balance sheet data for all US banks and, second, the bank office data which allows us to aggregate bank characteristics at the county level, depending on the share of deposits of a particular bank in total deposits of a particular county.

Bank balance sheet data

We use the Federal Financial Institutions Examination Council (FFIEC) Call reports to obtain the data on balance sheets of banks. The call reports are available at quarterly level but we average them to the annual level to match the frequency of the economic outcome variables from the CBP. As relevant bank characteristics we use bank size, capitalisation, liquidity position, and realised credit risk. We measure (i) the size of a bank using the average total assets; (ii) capitalisation using the leverage ratio (equity capital to total assets); (iii) the liquidity position using the share of short term non core funding as a percent of average total assets which implies that the higher the share the less liquid the bank; and finally (iv), we measure the realised credit risk using the share of loan charge-offs in total loans.

Figure 3.2: Bank characteristics

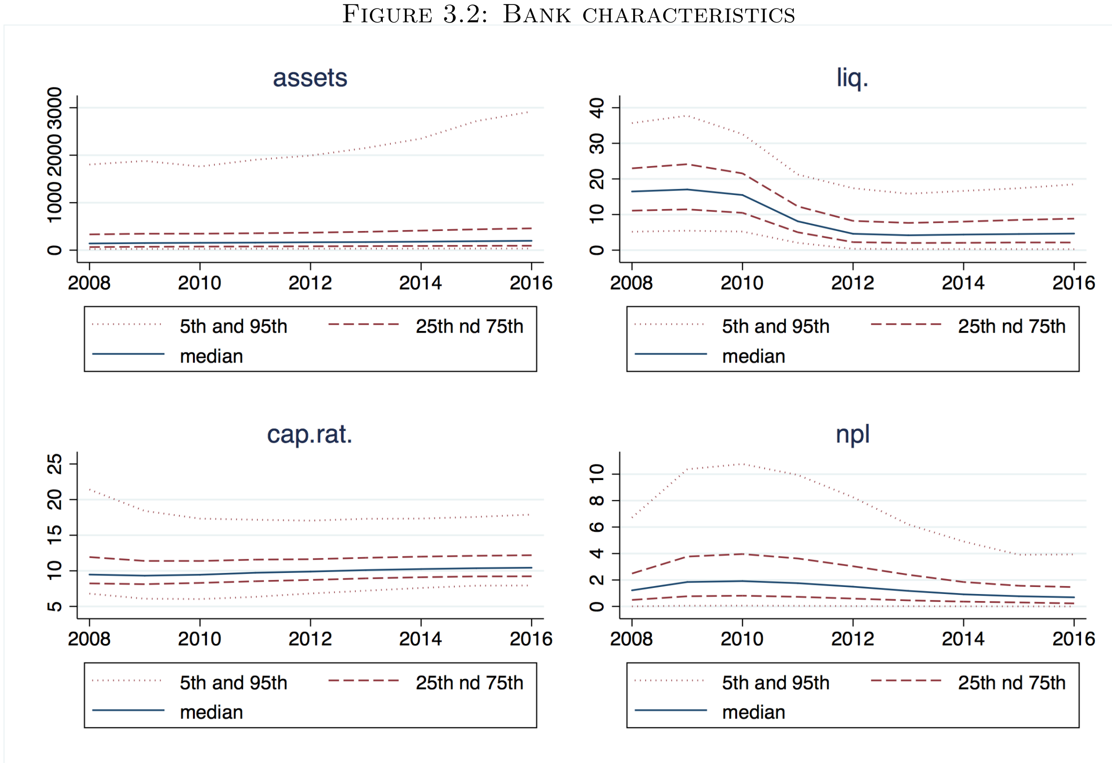


Figure 3.2 plots the distributions of bank characteristics over time. We can see a substantial amount of dispersion in the characteristics of banks. The 5th percentile bank held between \$14 billion and \$27 billion in total assests, while the median bank held between \$140 million and \$200 million. Dispersion in other characteristics was less pronounced, however it does exhibit a degree of countercyclicality. The weakest banks in terms of liquidity and realised credit risk were diverged from the median substantially more during financial crisis.

3.2.3 Probability of bank default

There is no single balance sheet item which alone predicts the riskiness of a bank, which is relevant for bank funding. To attain such a measure we use the characteristics of a bank to build a single measure which predicts the occurrence of a bank failure. We combine the balance sheet data of banks with the bank failure database administrated by the FDIC. Following the rationale of the CAMEL approach to monitoring bank riskiness we estimate the probability of a bank failure using the following logit model[1]:

$$\begin{aligned} logit(p(fail_{b,t}=1)) = \beta_0 + \beta_1 cap.rat_{b,t} + \beta_2 liq_{b,t} + \beta_3 npl_{b,t} \\ +\beta_4 noi_{b,t} + \beta_5 roa_{b,t} + \beta_6 nim_{b,t} + \epsilon_{b,t} \end{aligned} \tag{3.1}$$

where $fail_{b,t}=1$ denotes a failure of a bank, $cap.rat_{b,t}$ denotes the capital-to-assets ratio, $liq_{b,t}$ denotes the share of short term non core funding, $npl_{b,t}$ denotes the share of non performing loans,

[1] For a preview of the CAMEL approach, see Lopez (1999)

$noi_{b,t}$ denotes net operating income in total assets, $roa_{b,t}$ denotes the return on assets and $nim_{b,t}$ denotes the net interest margin. The estimates of this regression are shown in Table 3.1.

Table 3.1: Regression table

	(1)
	DEF
CAP.RAT	-0.583***
	(0.0211)
LIQ	0.0544***
	(0.00346)
NPL	0.0946***
	(0.00670)
NOI	0.216**
	(0.0908)
ROA	-0.297***
	(0.0904)
NIM	8.19E-08**
	(3.56E-08)
_CONS	-2.178***
	(0.179)
N	110653
$pseudoR^2$	0.4590

Standard errors in parentheses
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

As can be inspected, the reported estimates in Table 3.1 have the expected signs. In effect, low liquidity and poor portfolio contribute positively to the probability of a failure, while capitalisation and profitability contribute negatively. The model predicts in sample failures well. 96% of banks with the probability of default above the annual default rate experience a failure, while the pseudo Rˆ2 is 45%.

Figure 3.3: Estimated probability of bank failure

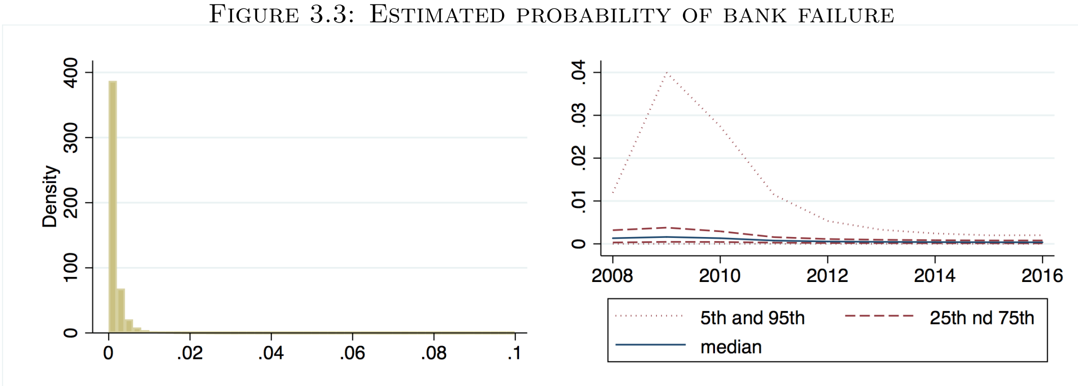


Figure 3.3 plots the distribution of the estimated probability of default (left panel) and the relevant percentiles of the distribution across time (right panel). Like balance sheet measures, the probability of default of a bank, exhibits countercyclical dynamics. Also, in periods of financial distress, the variance of the measure increases substantially implying that in bad times bad banks become worse relative to the average bank.

Office deposit data

In aggregating the bank level data to county level we use the shares of deposits by banks in total deposits in a particular area. The Federal Deposit Insurance Corporation (FDIC) Summary of Deposits data provides the data on the deposits held by every office of every bank in the United States. Precise geographical information on every office is also provided. This enables us to compute the amount of deposits that each bank holds in each county and for every county the share of that deposits each bank contributes to the total amount of deposits in this county. This will be our measure of the presence of a particular bank in a particular county. It can be argued that banks lend to counties other than those where they have offices. While this is true for a standardised products such as mortgage loans, Hoai-Luu (forthcoming) argues that this not the case for commercial loans.

Table 3.2: Bank offices statistics

	# of banks in a county			average # of offices per bank in a county			# of offices in a county			average maximum # of offices per bank in a county		
	mean	p50	sd	mean	p50	sd	mean	p50	sd	mean	p50	sd
2008	25	20	17	12	8	11	139	87	165	23	16	24
2009	25	20	17	12	8	12	140	89	165	24	16	28
2010	25	20	16	12	8	13	138	89	162	24	15	28
2011	24	20	16	12	8	13	138	88	162	23	15	27
2012	24	19	15	12	8	13	137	88	163	23	15	27
2013	24	20	15	12	8	13	136	86	162	23	15	28
2014	23	19	14	12	8	13	134	85	161	23	15	27
2015	23	19	14	12	8	13	132	83	158	22	14	27
2016	23	19	14	12	8	13	130	81	156	22	14	26

Table 3.2 provides the statistics of bank offices at a county level. An average county had between 25 and 23 banks operating at least one office in its area. In an average county an average bank operated 12 offices in that area. The biggest bank in an area on average operated between 24 and 22 offices in that area. The total number of offices per county varied between 130 and 140.

3.2.4 Local bank characteristics

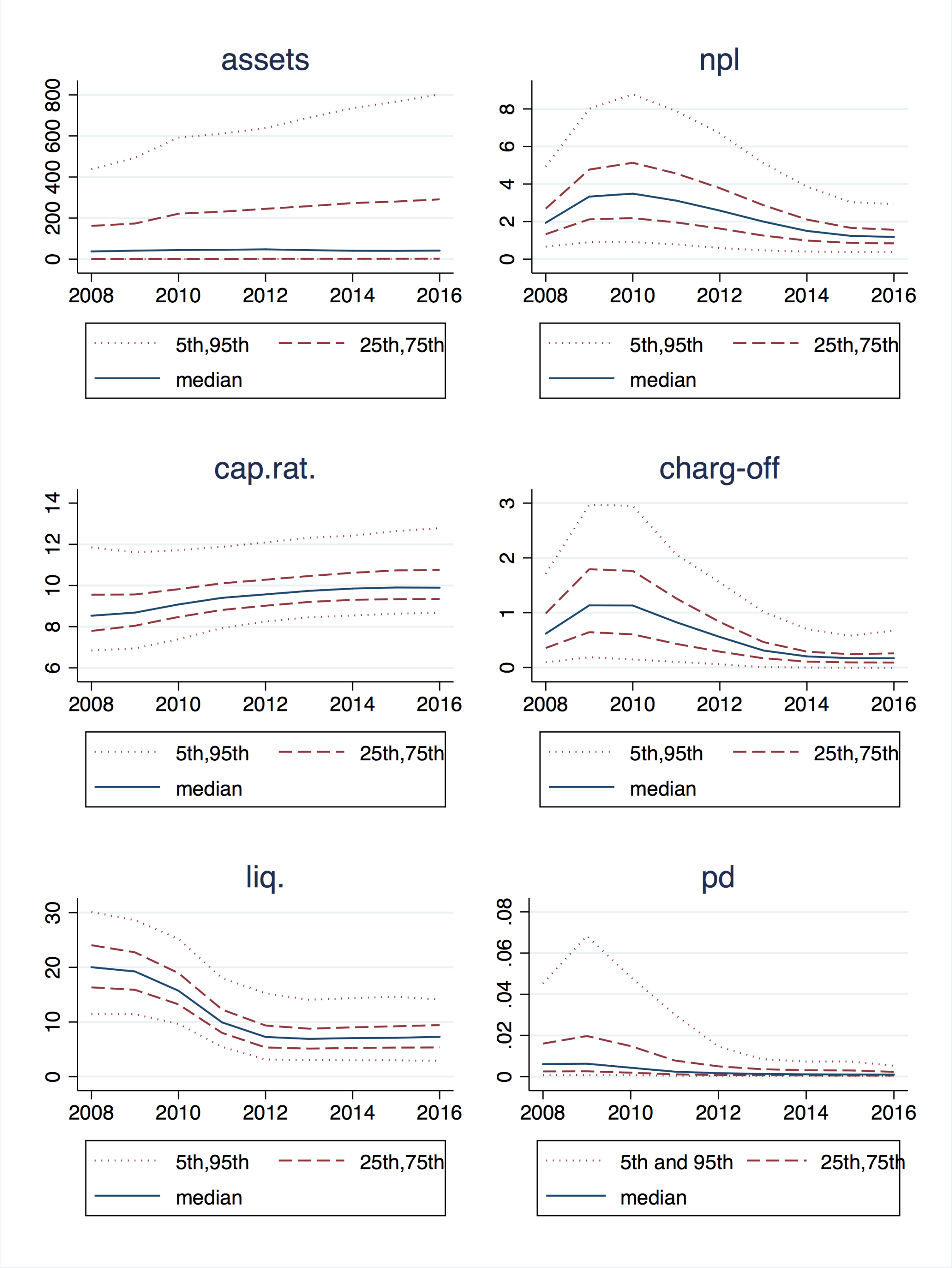
The data on bank offices with the geographical information of the offices allows us the calculate the share of each bank in total deposits for every county.

$$x_{g,t} = \frac{\sum_{b=1}^{B} dep_{g,b,t} x_{b,t}}{\sum_{b=1}^{B} dep_{g,b,t}} \tag{3.2}$$

where $dep_{g,b,t}$ denotes the total sum of deposits that the bank b holds in county g in period t, and $x_{b,t}$ is a characteristic of the bank b in period t. Among the latter are our measures of size, capitalisation, liquidity position of a bank, the realised risk and the probability of a bank default. Figure 3.4 plots the relevant percentiles of average local bank characteristics over time. As we move to variation of bank characteristics from bank level to the county level, the variance decreases somewhat, however it remains substantial.

We add two more local characteristics to our list of variables. First, for every bank, we compute the average share of deposits in the areas, where it operates and then use equation 3.2 to aggregate to a county level. This variable tells us how much market power banks, which operate in counties, have on average. Furthermore, for every county we compute the Hirschman−Herfindahl Index which measures the concentration of local markets. This will enable us to control for a possible confounding effect that market concentration might have on our results.

Figure 3.4: Local bank characteristics



3.3 Empirical strategy

We build our empirical analysis around measuring the effect that local bank characteristics have on the effect of monetary policy on local economic outcomes. As is standard in the literature, we use the federal funds rate a an indicator of monetary policy. We estimate the following model:

$$y_{g,t} = \beta_0 + \beta_1 fedfunds_{t-1} + \beta_2 x_{g,t} + \beta_3 fedfunds_{t-1} * x_{g,t} + \beta_4 controls_{g,t} + \eta_g + \epsilon_{g,t} \qquad (3.3)$$

where $fedfunds_{t-1}$ refers to the federal funds rate, $y_{g,t}$ refers to a measure of local economic outcome, described in section 3.2.1, and $x_{g,t}$ denotes local bank characteristic constructed as explained by equation 3.2. Neville et al. (2012) show the peak of the response of employment to a monetary policy shock lags between 7 and 9 quarters. We take this result into consideration and use a one year lag of federal funds rate in our analysis. Panel data structure allows us to control for the county specific effects, η_g. Other controls are discussed in the proceeding subsection.

What we are after is the change in local economic outcomes due to a change in monetary policy as a function of the local bank characteristics. The main coefficients of interest are therefore β_1 and β_3, more specifically, the term:

$$\frac{\partial y_{g,t}}{\partial fedfunds_{t-1}} = \beta_1 + \beta_3 * x_{g,t} \qquad (3.4)$$

We use logarithms of the local economic outcomes in equation 3.3, since counties differ in size in terms of employment and in terms of total annual pay. Regression in logarithms allows us to estimate the percent increase of local outcomes as a result of a percentage point increase in the federal funds rate.

3.3.1 Endogeneity and controls

There are two obvious sources of endogeneity which can arise in this setting. First, while local bank characteristics affect local economic outcome, local economic outcomes also affect bank balance sheets, introducing a problem of reverse causality. Second, aggregate demand affects local economic outcomes but also spurs a monetary policy reaction. If not controlled for, this would introduce the problem of omitted variable bias.

In addressing the first issue, the reverse effect of local economic outcomes on bank balance sheets, we are aided by the fragmentation of the US banking market and the way we construct our measures of local bank characteristics, where the share of a particular county in banks deposits is limited. To illustrate the point we compute the average share of deposits from counties for each bank. This indicates how much counties contribute to a bank on average. For every county we then compute the weighted average of that share. This brings the measure to the observation level of our analysis. Table 3.3 provides the statistics for this measure. For instance, for the county in the 75th precentile, the average bank had on average 24% of deposits from a single county. The data therefore suggests that large banks operate in large enough number of regions and this limits the market share of small local banks which concentrate in smaller number of regions.

Table 3.3: Distribution of average county shares in average bank deposits

	25th percentile	median	75th percentile
average county share	0.08	0.14	0.24

The second identified source of endogeneity is the omitted aggregate demand affecting the local economics outcomes and initiating a policy response. To control for this issue we add real GDP to the list of controls.

Finally, to address the issue of the effect of market power on our results, in the robustness checks we augment the regression with a measure of local market concentration and the interaction term of the local market concentration and the federal funds rate, thereby controlling for the confounding effect that market concentration is likely to have on our main results.

3.4 Results

Table 3.4: Regression table

	(1)	(2)	(3)	(4)	(5)	(6)
	log(emp)	log(emp)	log(emp)	log(emp)	log(emp)	log(emp)
$x =$	log(asset)	cap.r.	share	liq	chr.off	pd
L.log(rgdp)	6.920***	6.863***	6.919***	7.112***	6.694***	6.905***
	(0.377)	(0.373)	(0.375)	(0.385)	(0.373)	(0.376)
L.dr	-0.0153***	-0.0150***	-0.0149***	-0.0145***	-0.0145***	-0.0154***
	(0.00157)	(0.00158)	(0.00159)	(0.00158)	(0.00157)	(0.00158)
x	0.0155**	-0.00885***	-0.122*	0.00185***	0.0198***	0.131
	(0.00775)	(0.00329)	(0.0663)	(0.000672)	(0.00344)	(0.0995)
L.fedfunds	-0.135***	-0.211***	-0.173***	-0.173***	-0.150***	-0.165***
	(0.0331)	(0.0189)	(0.0109)	(0.0134)	(0.0110)	(0.0107)
interact	-0.00164	0.00506***	0.0285*	-0.000285	-0.00606***	-0.0924*
	(0.00169)	(0.00171)	(0.0153)	(0.000404)	(0.00155)	(0.0504)
year	-0.117***	-0.114***	-0.116***	-0.118***	-0.106***	-0.116***
	(0.00786)	(0.00773)	(0.00779)	(0.00780)	(0.00786)	(0.00779)
_cons	181.0***	174.9***	178.2***	180.4***	159.7***	177.9***
	(12.20)	(12.00)	(12.10)	(12.06)	(12.31)	(12.09)
N	3290	3290	3290	3290	3290	3290
R^2	0.585	0.587	0.585	0.588	0.591	0.585
adj. R^2	0.585	0.586	0.585	0.587	0.591	0.584

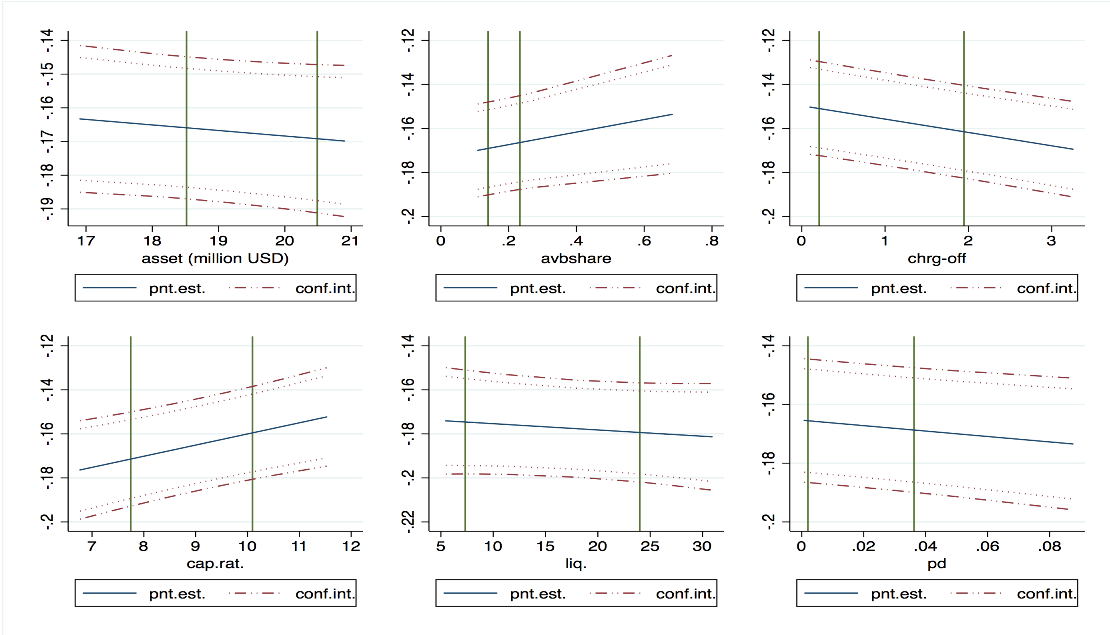
Standard errors in parentheses
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 3.4 provides the results for employment. Each of the columns provides the result for a different bank characteristic, as noted in the third line of the table. The table shows that included controls are statistically significant and exhibit an expected sign. Lagged aggregate real GDP affects local employment positively, while the lagged local mortgage default rate affects local employment negatively. Furthermore, the fed funds rate affects the local employment negatively. The interaction term is statistically significant at 10% level across all bank characteristics other than size and liquidity.

Figure 3.5 shows the percent change in employment due to a percentage point change in federal funds rate at different levels of local bank characteristics. The effect hovers at around -0.16, which implies that a one percentage point increase in the federal funds rate decreases employment by 0.16 percent. The dotted and the dashed lines indicate the 90% and the and the 95% confidence interval. Across all the measures the effect is negative. Green vertical lines indicate the 10th and the 90th percentile of the distribution of each of the bank characteristics.

Our results do not confirm the previous findings of Kashyap and Stein (1995, 2000) but instead show that bank size does not affect the effect of the federal funds rate negatively. Furthermore, the effect of the liquidity position of local banks on the effect of federal funds is also insignificant. The effect of the federal funds rate on local employment, however, does remain negative along the entire distribution of both the size of banks and the liquidity position of local banks. On the other hand, our findings suggest that as the capital structure increases and the realised credit risk and the

Figure 3.5: The effect of monetary policy on employment



probability of default decrease, the effect of the federal funds rate on local employment dampens. As the capitalisation of local banks increases from the 10th percentile, which is just below 8% of total assets to the 90th percentile, which is just above 10%, the effect of a percentage point increase in the federal funds rate increases from 0.16% to 0.17%. Similarly holds true when the probability of default decreases from the 90th percentile to the 10th percentile. The results also indicate that increased average market share of local banks dampens the effect of monetary policy.

Table 3.5 presents the results of the estimation for total annual payroll. As expected the results are similar to those for employment. The lagged real GDP increases local total annual payroll while the local default rates decrease it. The similarities are confirmed also in Figure 3.6. The effect of the federal funds rate is stronger than on the employment, -0.23. This implies that a one percentage point increase in federal funds rate decreases the total local annual payroll by 0.23 percent. As is the case of local employment, the effect is not affected by the size of local banks or their liquidity position. In both cases, as before, the effect of the federal funds rate on the local total annual payroll is negative along the entire distribution of the local bank characteristics. The effects of local market concentration, capitalisation, the realised credit risk and probability of a bank default on the effect of federal funds rate on local total annual payroll resemble strongly those in the figure on employment. Local market concentration and capitalisation of local banks dampen the effect of federal funds rate on local total payroll while the realised credit risk and the probability of default strengthen it.

Finally, Table 3.6 and Figure 3.7 present the results using local average annual payroll as an outcome variable. As before, lagged real GDP affects the average total annual payroll positively, while the local default rate affects it negatively. Most importantly, we find no effect of bank capitalisation or the concentration of local market on the effect of federal funds rate on the local average annual payroll, however, realised credit risk and the probability of failure of local banks intensify the negative effect of the federal funds rate on the outcome. In all cases the effect of federal funds rate on local average payroll is negative and statistically significant along the entire distribution of all local bank characteristics.

Table 3.5: Regression table

	(1)	(2)	(3)	(4)	(5)	(6)
	log(ap)	log(ap)	log(ap)	log(ap)	log(ap)	log(ap)
$x =$	log(asset)	cap.r.	share	liq	chr.off	pd
L.log(rgdp)	8.973***	8.896***	8.971***	9.144***	8.681***	8.953***
	(0.494)	(0.491)	(0.492)	(0.514)	(0.487)	(0.493)
L.dr	-0.0192***	-0.0188***	-0.0191***	-0.0185***	-0.0183***	-0.0193***
	(0.00191)	(0.00190)	(0.00193)	(0.00193)	(0.00191)	(0.00190)
x	0.0116	-0.0132***	-0.0723	0.00149*	0.0261***	0.278**
	(0.0100)	(0.00405)	(0.0818)	(0.000838)	(0.00440)	(0.110)
L.fedfunds	-0.209***	-0.291***	-0.240***	-0.246***	-0.209***	-0.229***
	(0.0477)	(0.0248)	(0.0146)	(0.0183)	(0.0140)	(0.0139)
interact	-0.00126	0.00674***	0.0341*	0.000101	-0.00855***	-0.175***
	(0.00245)	(0.00230)	(0.0188)	(0.000516)	(0.00192)	(0.0566)
year	-0.136***	-0.132***	-0.135***	-0.137***	-0.122***	-0.134***
	(0.0103)	(0.0102)	(0.0102)	(0.0103)	(0.0102)	(0.0102)
_cons	203.4***	195.9***	201.5***	203.5***	176.5***	199.5***
	(15.97)	(15.87)	(15.90)	(15.94)	(15.89)	(15.89)
N	3290	3290	3290	3290	3290	3290
R^2	0.743	0.745	0.743	0.744	0.747	0.744
adj. R^2	0.743	0.744	0.743	0.743	0.747	0.743

Standard errors in parentheses
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Figure 3.6: The effect of monetary policy on total annual payroll

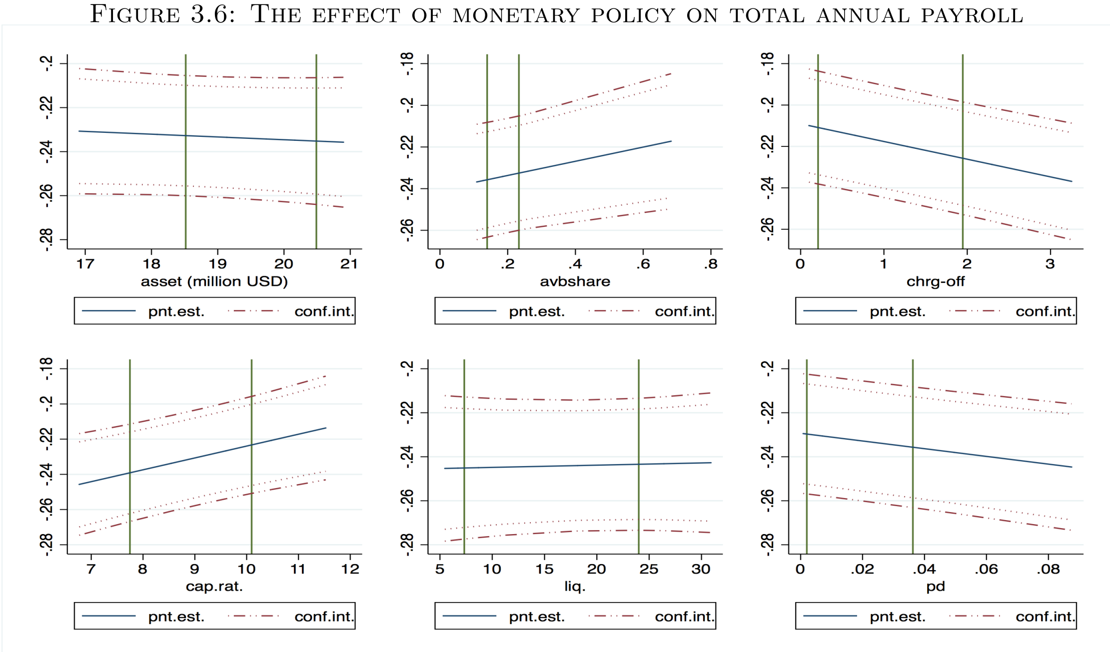
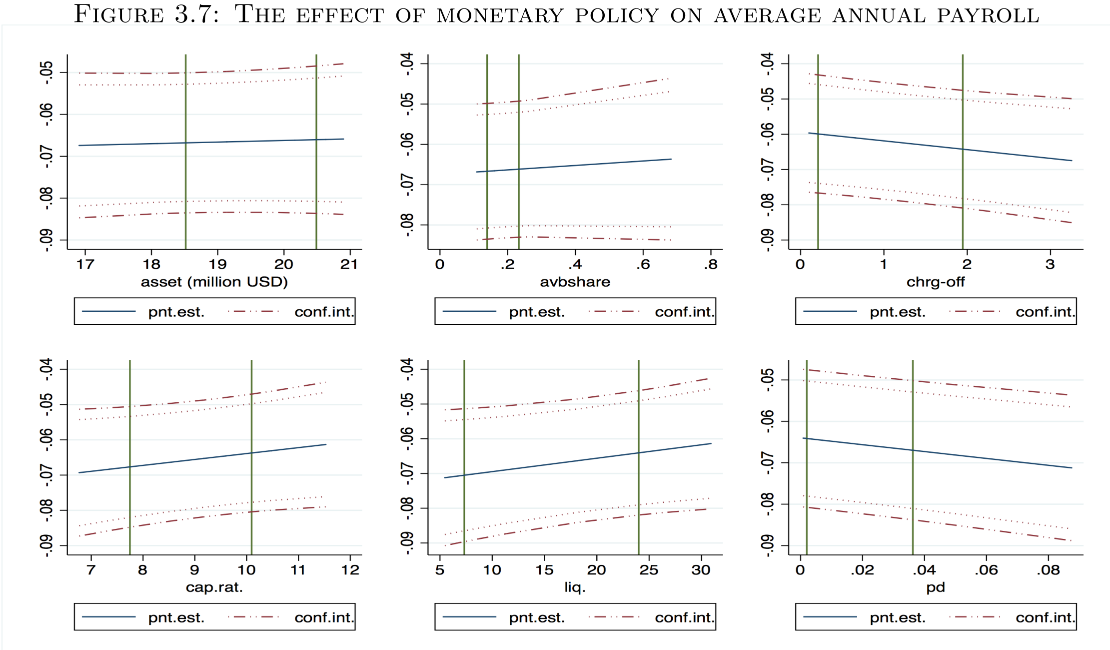


Table 3.6: Regression table

$x =$	(1) log(aap) log(asset)	(2) log(aap) cap.r.	(3) log(aap) share	(4) log(aap) liq	(5) log(aap) chr.off	(6) log(aap) pd
L.log(rgdp)	2.053***	2.034***	2.052***	2.032***	1.987***	2.049***
	(0.304)	(0.303)	(0.304)	(0.314)	(0.300)	(0.304)
L.dr	-0.00395***	-0.00380***	-0.00414***	-0.00403***	-0.00372***	-0.00398***
	(0.000928)	(0.000927)	(0.000926)	(0.000951)	(0.000933)	(0.000921)
x	-0.00396	-0.00433**	0.0499	-0.000362	0.00624***	0.147***
	(0.00490)	(0.00209)	(0.0346)	(0.000310)	(0.00225)	(0.0477)
L.fedfunds	-0.0739***	-0.0807***	-0.0675***	-0.0733***	-0.0594***	-0.0640***
	(0.0274)	(0.0156)	(0.00878)	(0.0107)	(0.00860)	(0.00848)
interact	0.000383	0.00168	0.00555	0.000386	-0.00249*	-0.0829***
	(0.00141)	(0.00141)	(0.0109)	(0.000289)	(0.00145)	(0.0318)
year	-0.0191***	-0.0183***	-0.0196***	-0.0194***	-0.0160**	-0.0187***
	(0.00638)	(0.00634)	(0.00637)	(0.00643)	(0.00627)	(0.00636)
_cons	22.45**	20.95**	23.31**	23.12**	16.87*	21.61**
	(9.931)	(9.856)	(9.911)	(9.942)	(9.772)	(9.893)
N	3290	3290	3290	3290	3290	3290
R^2	0.715	0.716	0.716	0.716	0.716	0.716
adj. R^2	0.715	0.716	0.715	0.715	0.716	0.716

Standard errors in parentheses
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

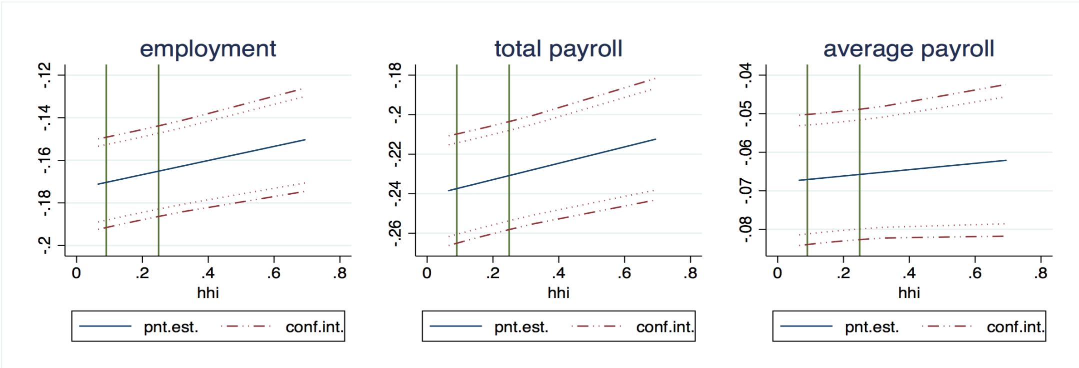
Figure 3.7: The effect of monetary policy on average annual payroll



3.5 Robustness - The confounding effect of the local banking market concentration

The data on deposits in offices of US banks also allows us to assess the concentration of local banking markets at the county level. Studies at the metropolitan area level have shown that the concentration of local banking market affects the transmission of monetary policy. Adams and Amel (2011), for instance, find that concentration of local markets dampen the effect of monetary policy on local lending. Figure 3.8 presents the results of the regression, where we substitute the local bank characteristics with a measure of local concentration, using the Herfindahl-Hirschman Index (HHI), both as a stand alone regressor and in the interaction like in the case of local bank characteristics in equation 3.3. As before, our main interest is in the effect that the HHI has on the effect of federal funds rate on the local economic outcomes. The regression results table can be found in the appendix.

Figure 3.8: The effect of HHI on the effect of monetary policy on local outcomes



As is the case in the previous chapter the federal funds rate affects all the local economic outcomes negatively across the whole distribution of the HHI. Furthermore, our results confirm previous findings that market concentration dampens the effect of monetary policy on both employment and total annual payroll. This result adds to that of Adams and Amel (2011) which use local lending as the outcome variable. Furthermore, our results extend their findings from the metropolitan area level to the county level. We cannot however confirm that the HHI has an effect on the impact of the monetary policy on the average annual payroll. To alleviate the issue of endogeneity we reestimate equation 3.3 adding the effect of the HHI and the interaction term between the HHI and the federal funds rate. All the results carry through. Higher average capitalisation, lower realised credit risk and lower probability of failure of local banks dampen the effect of the federal funds rate on both the employment and the total annual payroll, however the effect on the average annual payroll remains unaffected. The regression tables and the figures for the percent change in employment and payroll due to a one percent increase in the federal funds rate can be found in appendix 3.A.

3.6 Evaluation of the results

Our results are important in three dimensions. First, they confirm, that the monetary policy is effective in steering local economic outcomes, like employment, total annual payroll and the average annual payroll. Second, they shed light on the transmission channel of the monetary policy and the effect that bank characteristics have on the transmission. Finally, they add meaningfully to the discussion on the different effects that a common monetary policy has across different regions depending on the weakness of local banks.

Our results show, in a robust fashion across all specifications, that the monetary policy affects local economic outcomes as theory predicts regardless of the composition of local bank characteristics. We have estimated the effect across the entire distribution of values of bank characteristics. The point estimate of the decrease in employment varies between -0.15 and -0.17 percent due to a 1 percentage point increase in the federal funds rate. A tightening in monetary policy decreases both the employment and the total annual payroll, but the effect on the payroll is sufficiently strong to translate into a decrease in the average annual payroll, which decreases by around 0.07% due to a 1 percentage point increase in the federal funds rate.

Our results on the effect of local bank characteristics on the impact of monetary policy partially confirm findings in prior literature. Contrary to the predictions of Kashyap and Stein (1995, 2000) we find that as the capitalisation of local banks increases the the effect of monetary policy dampens. The mechanism for this result follows from predictions by Carlino and Defina (1998) who argue that it is not the size that enables banks to find alternative funding as deposits flow out of the banking system but rather their capital soundness. Similarly, we find that increased local banking market concentration and increased average market share of local banks dampen the effect.

Most importantly, our findings suggest that other bank characteristics, also affect the impact of monetary policy. We find that realised credit risk and probability of bank failure strengthen the impact of monetary policy. We rationalise this result as confirming the prediction that banks with easier access to alternative funding limit the outflow of deposits. Riskier banks, on the other hand, have limited access to wholesale funding and at higher cost which hampers their ability to obtain loanable funds. They reduce their loan supply and provide it at higher interest rate which translates into a stronger reaction of local economic outcomes.

Our findings support the view that the health of the banking system affects the functioning of the transmission mechanism. They also justify the calls for coordination between monetary and macroprudential policies. There has been a substantial amount of discussion on the coordination of the two policies in achieving the goals of financial stability and price stability, yet, the effect each of the two has on the mechanics of the other has been largely omitted. For instance, an important policy implication of our results suggests, that in the periods of economic expansions the monetary policy has to react in a stronger way if countercyclical capital buffers, which force banks to build up their capital, are in place.

There are several extensions of this paper which are subject to further research. The first, and most important, is to more carefully address the issue of endogeneity of the local bank characteristics. The fragmentation of the banking market provides evidence that the contribution of economic developments of counties to the performance of banks in their area is limited. A degree of that contribution nevertheless remains. The issue can get pronounced if banks mainly operate in clusters of counties where economic outcomes are highly correlated.

Furthermore, an extension of the analysis to the pre-crisis period would provide more variation of the federal funds rate which in the period analysed in this paper fluctuates near the zero lower bound. Pre-crisis period would also provide additional external validity to our results.

3.7 Conclusion

It is now a well established empirical and theoretical fact that monetary policy affects real output and employment and that an important channel of the transmission runs through credit and the banking system. Motivated by substantial differences in employment dynamics across different geographical areas and substantial differences across banks which operate in these locations, we estimate the effect that the characteristics of banks operating in a particular area have on the impact that the monetary policy has on the local economic outcomes.

We rely on the data on employment levels at the county level to measure local economic outcomes. We then aggregate the characteristics of banks to a county level using their share of deposits in total amount county deposits. According to our results, a tightening of the monetary policy decreases employment and total annual payrolls at county level. In line with the previous finding, the effect of monetary policy is dampened as the capital structure of local banks improve. This result goes in line with the prediction that large and well capitalised banks find it easier to attain alternative sources of funding after a monetary tightening. We add to this finding that also the risks associated with local banks affect local responses to monetary policy. As realised credit risk and probability of failure of local banks increases, the effect of monetary policy intensifies. We rationalise this result as indicating that the riskiness of the bank also determines its access to alternative sources of funding in periods of monetary tightening. We find no evidence which might suggest that the size of local banks or their liquidity position affects the transmission of monetary policy.

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3.A Controlling for HHI

Table 3.7: Regression table

	(1)	(2)	(3)	(4)	(5)	(6)
	log(emp)	log(emp)	log(emp)	log(emp)	log(emp)	log(emp)
$x =$	log(asset)	cap.r.	share	liq	chr.off	pd
L.log(rgdp)	6.919***	6.860***	6.923***	7.125***	6.687***	6.907***
	(0.378)	(0.374)	(0.374)	(0.385)	(0.374)	(0.377)
L.dr	-0.0152***	-0.0150***	-0.0149***	-0.0145***	-0.0146***	-0.0153***
	(0.00157)	(0.00158)	(0.00160)	(0.00158)	(0.00158)	(0.00158)
x	0.0148*	-0.00992***	-0.111	0.00202***	0.0206***	0.131
	(0.00772)	(0.00336)	(0.0823)	(0.000658)	(0.00345)	(0.0990)
L.fedfunds	-0.157***	-0.215***	-0.171***	-0.175***	-0.152***	-0.171***
	(0.0337)	(0.0192)	(0.0110)	(0.0133)	(0.0111)	(0.0109)
interact	-0.000780	0.00490***	-0.0411	-0.000475	-0.00785***	-0.0829
	(0.00170)	(0.00172)	(0.0300)	(0.000404)	(0.00164)	(0.0504)
hhi	-0.0519	-0.0858*	-0.0104	-0.0648	-0.0597	-0.0591
	(0.0462)	(0.0481)	(0.0586)	(0.0437)	(0.0425)	(0.0465)
hhi*fedfunds	0.0326***	0.0326***	0.0675***	0.0400***	0.0396***	0.0322***
	(0.0115)	(0.0122)	(0.0234)	(0.0112)	(0.0106)	(0.0113)
year	-0.117***	-0.114***	-0.116***	-0.118***	-0.105***	-0.116***
	(0.00788)	(0.00775)	(0.00779)	(0.00782)	(0.00789)	(0.00780)
_cons	180.7***	174.2***	178.4***	180.2***	158.9***	177.8***
	(12.23)	(12.03)	(12.10)	(12.10)	(12.37)	(12.12)
N	3290	3290	3290	3290	3290	3290
R^2	0.586	0.588	0.586	0.590	0.593	0.586
adj. R^2	0.585	0.587	0.585	0.589	0.592	0.585

Standard errors in parentheses
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Figure 3.9: The effect of monetary policy on employment

Table 3.8: Regression table

$x =$	(1) LOG(AP) LOG(ASSET)	(2) LOG(AP) CAP.R.	(3) LOG(AP) SHARE	(4) LOG(AP) LIQ	(5) LOG(AP) CHR.OFF	(6) LOG(AP) PD
L.LOG(RGDP)	8.973***	8.900***	8.981***	9.156***	8.674***	8.957***
	(0.494)	(0.491)	(0.491)	(0.514)	(0.487)	(0.493)
L.DR	-0.0193***	-0.0189***	-0.0190***	-0.0186***	-0.0184***	-0.0194***
	(0.00190)	(0.00190)	(0.00195)	(0.00194)	(0.00191)	(0.00190)
X	0.0116	-0.0135***	-0.117	0.00167**	0.0271***	0.272**
	(0.01000)	(0.00411)	(0.109)	(0.000813)	(0.00442)	(0.110)
L.FEDFUNDS	-0.239***	-0.293***	-0.239***	-0.248***	-0.213***	-0.236***
	(0.0495)	(0.0251)	(0.0146)	(0.0184)	(0.0141)	(0.0143)
INTERACT	-0.0000707	0.00611***	-0.0466	-0.000167	-0.0108***	-0.162***
	(0.00251)	(0.00229)	(0.0420)	(0.000517)	(0.00214)	(0.0562)
HHI	0.0144	-0.0286	0.0597	0.00254	0.00658	0.00681
	(0.0625)	(0.0635)	(0.0839)	(0.0595)	(0.0555)	(0.0619)
HHI*FEDFUNDS	0.0420***	0.0416**	0.0800**	0.0456***	0.0512***	0.0398**
	(0.0158)	(0.0166)	(0.0352)	(0.0155)	(0.0151)	(0.0156)
YEAR	-0.137***	-0.132***	-0.136***	-0.137***	-0.121***	-0.134***
	(0.0103)	(0.0102)	(0.0102)	(0.0103)	(0.0102)	(0.0102)
_CONS	203.5***	195.8***	201.6***	203.5***	175.9***	199.7***
	(15.96)	(15.86)	(15.91)	(15.93)	(15.86)	(15.89)
N	3290	3290	3290	3290	3290	3290
R^2	0.744	0.745	0.744	0.745	0.748	0.744
ADJ. R^2	0.743	0.744	0.743	0.744	0.747	0.743

Standard errors in parentheses
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Figure 3.10: The effect of monetary policy on total annual payroll

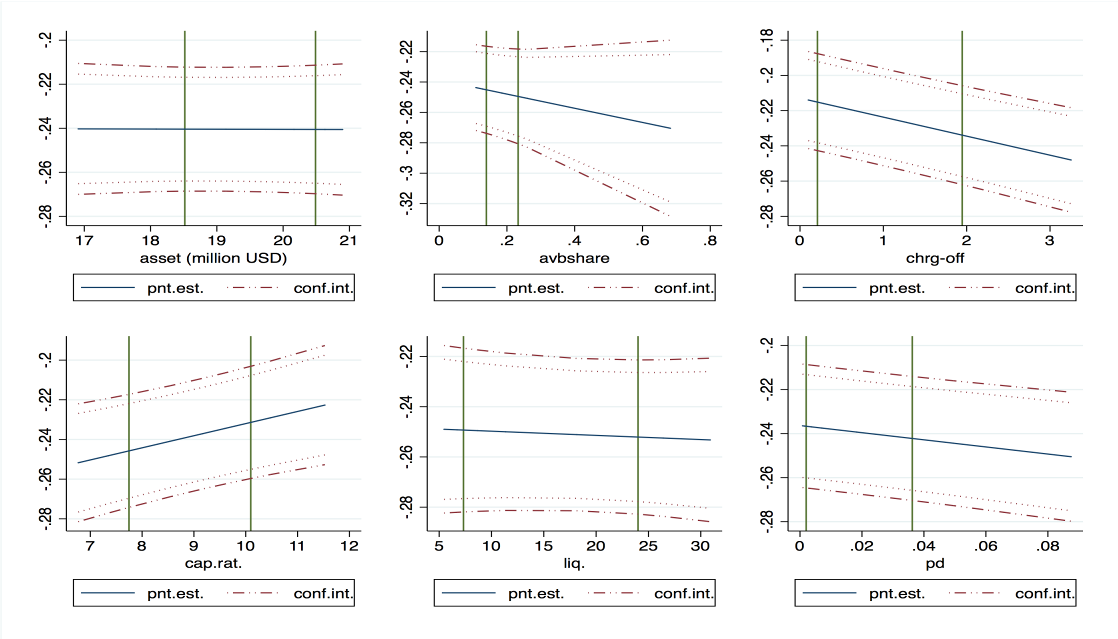

Table 3.9: Regression table

	(1)	(2)	(3)	(4)	(5)	(6)
	log(aap)	log(aap)	log(aap)	log(aap)	log(aap)	log(aap)
$x =$	log(asset)	cap.r.	share	liq	chr.off	pd
L.log(rgdp)	2.054***	2.039***	2.058***	2.032***	1.986***	2.050***
	(0.305)	(0.303)	(0.304)	(0.314)	(0.301)	(0.304)
L.dr	-0.00403***	-0.00391***	-0.00402***	-0.00413***	-0.00382***	-0.00407***
	(0.000923)	(0.000923)	(0.000933)	(0.000947)	(0.000929)	(0.000917)
x	-0.00323	-0.00362*	-0.00580	-0.000344	0.00646***	0.142***
	(0.00492)	(0.00210)	(0.0460)	(0.000304)	(0.00226)	(0.0490)
L.fedfunds	-0.0818***	-0.0784***	-0.0675***	-0.0727***	-0.0605***	-0.0655***
	(0.0287)	(0.0157)	(0.00883)	(0.0109)	(0.00866)	(0.00860)
interact	0.000709	0.00121	-0.00554	0.000308	-0.00294*	-0.0790**
	(0.00145)	(0.00141)	(0.0263)	(0.000291)	(0.00155)	(0.0317)
hhi	0.0663**	0.0572**	0.0700**	0.0674**	0.0662***	0.0659***
	(0.0257)	(0.0260)	(0.0341)	(0.0261)	(0.0243)	(0.0253)
hhi*fedfunds	0.00931	0.00897	0.0124	0.00564	0.0116	0.00756
	(0.0103)	(0.00994)	(0.0232)	(0.00961)	(0.0104)	(0.00978)
year	-0.0193***	-0.0186***	-0.0195***	-0.0195***	-0.0161**	-0.0189***
	(0.00639)	(0.00633)	(0.00638)	(0.00643)	(0.00627)	(0.00636)
_cons	22.80**	21.56**	23.22**	23.33**	16.98*	21.95**
	(9.937)	(9.854)	(9.935)	(9.952)	(9.771)	(9.899)
N	3290	3290	3290	3290	3290	3290
R^2	0.716	0.717	0.716	0.716	0.717	0.717
adj. R^2	0.716	0.716	0.716	0.716	0.717	0.716

Standard errors in parentheses
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Figure 3.11: The effect of monetary policy on average annual payroll